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SCIENCE, METHODOLOGY  
AND RELIGION  
IN THE WORK OF  
ADAM SEDGWICK

PhD Thesis  
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*A Sedgwick*

ADAM SEDGWICK, ÆT. 82.

*a crayon drawing by Lorraines Dickinson*



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Thanks are due also to other libraries for help and for access to their published materials, including: the Open University, the Universities of Lancaster, London and Manchester, The Evangelical Library, Dr Williams Library, Chetham's Library, The Manchester Central Reference Library, Kendal Public Library. In my own Lancashire Polytechnic library, Mr. M. Peel and Ms. H. Higgins have been especially helpful in locating published materials.

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Finally, I would like to thank my wife and parents for help in preparing the manuscript, and all my family for doing without me for long periods of time during my work on it.

V. Paul Marston  
1984.

## SCIENCE, METHODOLOGY AND RELIGION IN THE WORK OF ADAM SEDGWICK

An abstract of a PhD. Thesis by V. Paul Marston.

Adam Sedgwick (1785-1873) was one of the leading geologists in the second quarter of the nineteenth century, and played a major part in establishing the geological column and applying it in the U.K. This thesis examines the metaphysical, religious and methodological presuppositions implicit in his approach to science.

A prevailing view amongst historians (e.g. Cannon and Morrell) has seen Sedgwick as a 'liberal Anglican' and 'Broad Churchman'. This has been linked with a view connecting the vanguard of science in the period with liberal Anglicanism (e.g. in the BAAS), and seeing both Tractarians and to a large extent Evangelicals as a 'threat' to it. This thesis presents evidence showing the inadequacy of the 'Broad Church' concept, and that Sedgwick is himself closer to an Evangelical position than has been imagined. It shows that his presuppositions about the nature of science and its relationship with religion were close to those of Scottish Evangelicals like Chalmers and Miller, and not dissimilar from the leading moderate Anglican Evangelicals who would have associated with the Christian Observer. The influence of Coleridgean liberalism was small.

Sedgwick also contributed to the development of Natural Theology in a time when it was in ascent. Evidence shows that criticisms of Sedgwick for semi-deism (e.g. by Hooykaas) are unfounded, and that his natural theology was consistent with a full Christian theism.

Finally, the thesis examines Sedgwick's participation in the nineteenth century debates about scientific methodology. It shows that, having taken as his mentors on the issue Bacon and Newton, Sedgwick's thinking evidences a certain tension as he tries to interpret what he is actually doing in science in these terms.

NOTE

Frequent reference has been made in this thesis to the monumental The Life and Letters of the Reverend Adam Sedgwick, by John Willis Clark and Thomas McKenny Hughes (2 vols., Cambridge, 1890). This has been referred to as 'Clark & Hughes' both in the text and in the notes. Frequent citation has also been made of various works of Sedgwick, and these may be referred to in a shortened form, e.g. 'Discourse' for A Discourse on the Studies of the University (Cambridge, 1833). Full reference to these and other works may be found in chapter 10.3 of the Bibliography.

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## (1) INTRODUCTION

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The Reverend Adam Sedgwick (1785-1873) was the Woodwardian Professor of Geology at Cambridge from 1818-1873. He was one of the best field geologists of his generation, and in that heroic age of geology did perhaps more than any other individual towards the construction of a geological column. He was also active in founding the Cambridge Philosophical Society in 1819, and as one of the early leaders in the British Association for the Advancement of Science founded in 1833. The present thesis concentrates on an analysis of his basic theological and philosophical position, relating it both to his science and to the background of his times. The latter has inevitably involved some analysis of the various different philosophical and more especially religious parties of the period.

The approach taken in this thesis to analysis of contemporary viewpoints is 'instrumentalist' in the sense that viewpoints (as distinct from parties with a specific membership) need not be assumed to correspond with any kind of 'external reality'. Terms like 'Deist' or 'Broad Church' or 'Evangelical' or 'Cambridge Network' may be seen simply as instruments which may (or may not) help analysis of past situations, and sometimes predictions of positions which individuals might have been expected to take up. The approach is also in a sense 'actualistic' in that (with obvious qualifications and recognising the limitations) some present theological or metaphysical contentions may help us penetrate the thinking behind some past ones. Finally, the thesis makes neither exclusively externalist nor exclusively internalist assumptions about historical processes.

Amongst modern historians of science who are interested in issues of science and religion in the nineteenth century, the



general trend has been to follow Conybeare's identification of three main parties within the Church of England: High Church, Evangelical, and Broad Church.<sup>1</sup> Since at least 1940<sup>2</sup> Sedgwick has been identified with liberal Anglicanism of the Broad Church. Following this, Gillispie and Hooykaas, leading historians of science (particularly of geology) in the 1950's and early 1960's, emphasized a supposedly strong distinction between the theology of Sedgwick and of the Evangelical Hugh Miller.<sup>3</sup> During the 1960's W.F. Cannon, himself critical of Gillispie and Hooykaas<sup>4</sup>, made an emphasis on supposed 'Broad Church' influence on early nineteenth century science which continues to be evident in recent literature. Cannon associated with the 'Broad Church' a group he called the 'Cambridge Network': 'a loose convergence of scientists, historians, dons and other scholars with a common acceptance of accuracy, intelligence and novelty.'<sup>5</sup> These men were seen in various areas: 'in religion as Broad Church leaders, in literature as propagandists for the Romantic poets, in historiography as "Liberal Anglicans," in education as University reformers, and in the history of cliques as related to the Cambridge "Apostles"'.<sup>6</sup> Cannon continues that in private communications: 'Robert Robson, Martin Rudwick, and I have agreed that these are all one set of people with the same ideals, operating in all these fields and in science as well.' Whilst many might think that this really should have settled it, there are some of us who doubt it.<sup>7</sup> But the Cannon thesis has been influential. It identifies a 'scientific node' of the 'Cambridge Network', consisting of Cambridge and ex-Cambridge scientists and a few others (sometimes called 'friends of the Network'), and a 'theological node' centred on Julius Hare. The movement is supposed to have stemmed from Coleridge, taking

Niebuhr's approach to history but seeing Strauss as a poor application of that approach.

Following Cannon, Thackray and Morrell have emphasized the influence in the early British Association (BAAS) and in science of the period of this 'Cambridge Network' - identifying it as 'a set of liberal Anglican, or Broad Church, proponents of moderate reform who found one spiritual home in Trinity College, Cambridge.'<sup>8</sup>

Thackray and Morrell assert:

'Trinity College was a seminary of the Broad Church or liberal Anglican Movement. J.C.Hare and Connop Thirlwall, who were Trinity tutors, helped to articulate that blend of Kantian idealism, toleration, and Coleridgean conservatism which came to characterize the Movement, and which appeared as much in the scientifico-moral tracts of Whewell as in the Movement's more directly theological and political writings. The liberal Anglican theology of the 'Cambridge network' rapidly became the unofficial credo of the BAAS, with important ideological and practical results.'<sup>9</sup>

John Hedley Brooke, in various papers, seems to assume the Cannon concepts<sup>10</sup>, and Brooke and Morrell have further developed the idea that natural theology within BAAS served both to allay some clerical criticism and to act as a mediating influence between those of different church allegiances.<sup>11</sup> Others accept the Cannon thesis as established.<sup>12</sup> The recent biography of Sedgwick by Colin Speakman takes for granted the 'Cambridge Network' with its 'Broad Church' orientation, and portrays Sedgwick as a 'Broad Church radical'.<sup>13</sup>

Cannon, Thackray and Morrell rightly emphasize Sedgwick's importance. He was 'one of the world's best geologists'<sup>14</sup> in an era when the Cambridge Network's power base depended on the

geologists, and an axis of geologists 'was crucial in shaping a sense of common purpose among the Gentlemen of Science.'<sup>15</sup> Moreover the British Association (BAAS) 'depended heavily on its star performers and orators of whom Sedgwick was the most renowned.'<sup>16</sup> He was 'the most vigorous liberal in the University'<sup>17</sup> and 'a principal agent in promoting Darwin.'<sup>18</sup>

Now, as we have seen, the supposed 'Network' is repeatedly called 'liberal Anglican' and 'Broad Church'<sup>19</sup>, with a 'liberal Anglican' or 'Latitudinarian' theology.<sup>20</sup> Thackray and Morrell say:

'The leaders were not only practising Anglicans but were deeply embedded in Anglican life; no fewer than ten were ordained, and five of the ten were sons of Anglican clergy (Buckland, Harcourt, Lloyd, Peacock, Sedgwick). Their theology was that of a distinct party within the Church of England, the 'liberal Anglican' or Broad Church party. Edward Stanley was a noted publicist of this party and became a widely influential bishop. Baden Powell was also active in the development of liberal Anglican theology. Many other Broad Church ramifications can be traced within the core group, such as James David Forbes's enthusiasm for the ideas of Thomas Arnold and George Biddell Airy's sympathy for Bishop Colenso. The British Association's ideology of science was everywhere formed by Broad Church sympathies.'<sup>21</sup>

Here we pass from Sedgwick to extreme radicals like the Powell and Colenso of the 1860 Essays and Reviews, as though they were all one harmonious group. But then, Sedgwick is specifically called a 'liberal Anglican'.<sup>22</sup>

The movement is assumed (in Arnold's words) to have 'stemmed from Coleridge'<sup>23</sup>. Coleridge is stated to have been very important

in the earliest BAAS meetings, and significance is attached to his part in Whewell's coining the term 'scientist'.<sup>24</sup> The leaders of the movement were 'ardent admirers of Wordsworth and Coleridge as intuitive thinkers' - which Cannon sees as the basis of Sedgwick's attacks on Payleyan ethics.<sup>25</sup> Sedgwick's Discourse on the Studies of the University (1833) 'with its attacks on Locke and Paley, was of considerable importance in defining the new romantic-liberal-scientific stance; and Sedgwick kept up his interest by perusing writings of such younger men as Arthur Stanley, F.D. Maurice, and Leopold von Ranke, as well as those of his old friend Julius Hare.'<sup>26</sup>

The present thesis makes and documents a number of assertions about the prevailing Cannon-Thackray-Morrell assumptions. First, the theological and philosophical position of Adam Sedgwick has been radically misunderstood. It is misleading to describe him either as 'Broad Church' or 'liberal Anglican', rather, his first serious biographers Clark and Hughes were more accurate in having: 'Sedgwick, if he belonged to any party in the Church, an Evangelical.'<sup>27</sup> His personal religion was Evangelical, and his approach to key issues of the atonement, the evidences for Christianity, natural theology, and the rejection of Payleyan ethics were all Evangelical rather than Coleridgean, and drew either on actual Evangelical sources or on sources favoured by Evangelicals. Sedgwick was widely read, was not a 'party man' and would find stimulation in ideas and information from any source; but his specific statements of theological agreement are usually to Evangelicals like Simeon, Hall and Chalmers.<sup>28</sup>

But there are other points to be made, more general than the misunderstanding of one of the key figures in the analysis. One

of these is the misrepresentation (albeit implied rather than specifically stated) of Evangelical opinion as largely hostile or at best indifferent to science. Evangelicalism is usually only ascribed by Cannon or Thackray and Morrell to individuals or periodicals at times when they are seeming critical of science or scientists. Thus Cannon cites the pioneering work in science teaching in the Universities of Milner, Farish, Clarke and Walker<sup>29</sup> - but does not tell us that Milner and Farish were leading Evangelicals, nor that Clarke chose to identify with them in the inaugural Bible Society meeting of 1812.<sup>30</sup> Faraday is 'the contemporary scientist most admired by the Network'<sup>31</sup>, but his very conservative religious views are passed over or noted as an aberration of mystery. Thomas Arnold (whom virtually everyone who applies the term 'Broad Church' within the period takes to be a central member of it) apparently did not think highly of science.<sup>32</sup> When arch-Evangelical Simeon expresses very similar views it is seemingly more significant.<sup>33</sup> Then, 'Evangelical Christians, given to a literal interpretation of Genesis, led the assault (on the BAAS)',<sup>34</sup> Nolan and Cockburn (who as we shall see were less orthodox in their Evangelicalism than Sedgwick on certain issues, and certainly did not speak for the Simeonite leaders) are presented as though typical. On the other hand the vindication of 'the liberal Anglican position' (my underline) is supposed to have come from Powell and Daubeny,<sup>35</sup> with Powell merely 'going further than any liberal Anglican divine had dared.'<sup>36</sup> There is no indication here that Powell, far from simply being daring enough to do what the others would have liked to had they been not too timid, went even further by 1860 than most of the very radical contributors to Essays and Reviews in his apparent rejection of miracles. Yet Thackray and Morell cite the Evangelical

Christian Observer here just in its criticism of the radical Powell

- no one would gather that in fact it had consistently supported mainstream geology and been critical of so called Scriptural

Geologists.<sup>37</sup> Even poor Mary Buckland is introduced to laugh

at Nolan - but with no mention of her own friendship with Pusey,

or attendance at an Evangelical Church.<sup>38</sup> Chalmers' Evangelicalism

is noted , but then in the next sentence he (probably the most renowned Evangelical outside Anglicanism) is slipped in as one of

the authors who 'confirmed the close alliance between the British

Association and the liberal natural theologians'.<sup>39</sup> In short, all the

links with Evangelicalism of the supposed 'Gentlemen of Science'

are ignored or played down, and a picture presented as though

Evangelicals in general were ranting bibliolators or at best

indifferent to science. The relationships between moderate

Evangelicals and science could be totally misunderstood by

those reading such treatments.

A third major area of criticism to be made of what has been here called the 'Cannon-Thackray-Morrell' approach, is its use of the concept of the 'Broad Church'. The many caveats placed on the use of the term by earlier writers are glossed over, and the term used as though it really could be applied to a group of people with a common theology. But this retrospective application of a term invented in the 1850's to describe a highly heterogenous set of people in the 1820's and 1830's confuses the issue. The theology of (say) Thirlwall or Whately differed radically from that of (say) Coleridge or Maurice - and the theology of Sedgwick (and probably others like Whewell) differed radically from either. Moreover the term 'Cambridge Network' is itself of dubious value. Does it imply much more than the unexceptional fact that in years when there were but two

old established universities, many of England's most conspicuous intellectual talents - especially in the Anglican Church - passed through or were affected by one of them? If it implies much more than this - say to some shared philosophy or religion not found in other Anglicans - then it is probably better dropped.

Concepts are only instruments. On the whole, and especially with regard to some modern historians of science<sup>40</sup> this thesis is arguing for a change in perspective rather than acknowledgement of specific items of fact. One may be reminded of what Kuhn once said of Popper: 'How am I to persuade Sir Karl, who knows everything I know about scientific development, and has somewhere or other said it, that what he calls a duck can be seen as a rabbit.'<sup>41</sup> Yet this change in perspective is significant, and impinges also on our understanding of Sedgwick's opposition to evolution.

On another level, the thesis also examines in depth the methodological presuppositions Sedgwick made in pursuing his science. These relate not only to his understanding of the relationship between religious and scientific knowledge, but also to his perception of what he was doing during what we might now describe as paradigm change and development.

This thesis, then, concentrates primarily on the philosophical and theological ideas of Sedgwick in relation to his science.<sup>42</sup> Yet, in doing so, it seeks to challenge a number of current ideas and perspectives on the relationship between science and religion in the nineteenth century. In particular, it may furnish some contribution to the recognition of the influence of moderate Evangelicals and their ideologies on the development of science.

1. W.D. Conybeare, 'Church Parties', in Essays Ecclesiastical and Social (5th Edn., 1855, pp. 57-164) is analysed in chapter 2.3 below.
2. D.A. Winstanley, Early Victorian Cambridge, p. 94.
3. C.C. Gillispie, Genesis and Geology (1951), and R. Hooykaas, The Principle of Uniformity in Geology, Biology and Theology (1963).
4. e.g. of Gillispie in 'The Problem of Miracles in the 1830's', Victorian Studies, 1960-1, 4, 5-32 (note 3).
5. S.F. Cannon, Science in Culture, p. 30, and see also 'Scientists and Broad Churchmen', British Studies, 1964, 4, 65-88.
6. Cannon, Science in Culture, p. 30.
7. Pietro Cosi pointed out to me his own scepticism that this was sufficient in his Isis review of the book (1979, 70, 593-5). J.B. Morrell's review in History of Science (1980, 18, 39-45) is much less critical, and Morrell's own work adopts Cannon's views. D.M. Knight's review (Nature, 1978, 276, 131-2) is more critical of its limitations but finds it 'illuminating' as an account of the 'Cambridge Network'. Michael Ruse (in Victorian Studies, 1979-80, 23, 118-119) is more ecstatic, finding the essay on the Cambridge Network 'masterly'.
8. A. Thackray and J.B. Morrell, Gentlemen of Science, p. 21.
9. Ibid., p. 226.
10. e.g. in 'Natural Theology and the Plurality of Worlds', Annals of Science, 1977, 24, 221-286.
11. Brooke in his chapter in Images of the Earth (Ed. L. Jordanova and R. Porter), and Morrell in his review (ref. 7).
12. E.g. in R. McCleod and P. Collins (Eds.) Parliament of Science,



(1981) p. 8 and p. 16 n32; I. Inkster and J.B. Morrell (Eds) Metropolis and Province (1983) p. 199 n10; D. Miller, 'Between Hostile Camps', BJHS, 1983, 16, 1-47 (p. 13); T Cosslet (Ed.) Science and Religion in the Nineteenth Century (1984), p. 13 n51. The notable exception is N. Rupke, The Great Chain of Being (1983), which totally ignores Cannon's 'Cambridge Network' and makes Oxford the centre of English Geology in the 1830's. I believe Rupke exaggerates Buckland's importance in this period, and to regard Sedgwick after 1830 as in any basic sense a member of a Bucklandian 'Oxford School' in antithesis to a Scottish one, leads to anomalies. One obvious example of this is Rupke's contrast on p. 162 between the ideas of Buckland and those of Agassiz and Miller on progressivism. Sedgwick is unmentioned, but his clear self-alignment with the 'wrong' side on this issue is just one of a number. At the time of writing this thesis, however, Rupke's ideas, unlike the Cannon 'Cambridge Network', had not gained any wide support, and I have not analysed them in detail.

13. C. Speakman, Adam Sedgwick (1982), refers to the 'Cambridge Network' (p. 90), and to Sedgwick's 'Broad Church radicalism' (p. 35); but he also refers to a 'puritanical streak' (p. 31) and to a 'low church directness' in Sedgwick's sermons (p. 134)!

14. Cannon (ref. 6), p. 40.

15. Thackray and Morrell (ref. 8), p. 27.

16. Ibid., p. 161.

17. Cannon (ref. 6), p. 54.

18. Ibid., p. 56.

19. Thackray and Morrell (ref. 8), pp. 21, 25, 101, 124, 225, 228, 245.

20. Thackray and Morrell use the term 'Latitudinarian' on Ibid., p. 28.
21. Ibid., p. 25.
22. Ibid., p. 245.
23. Cannon (ref. 6), p. 30.
24. Thackray and Morrell (ref. 8), p. 20 and 246. They state  
 'It is a point of some significance that Coleridge was lionized  
 at the 1833 Cambridge Meeting of the British Association, where  
 he forbade the members to call themselves philosophers. In  
 response, William Whewell coined the word 'scientist' to designate  
 collectively those who studies material nature.' (p. 20)
25. Cannon (ref. 6), p. 59.
26. Cannon (ref. 6), p. 40.
27. Clark & Hughes, 1, p. 220.
28. Simeon, Chalmers and Hall are, arguably, the greatest amongst  
 Evangelicals of the Anglicans, Church of Scotland and Baptists  
 in the early part of the nineteenth century.
29. Cannon (ref. 6), p. 32-3, 36 and 42.
30. Though Cannon does repeat the story of Clarke's apparent  
 preoccupation with his work in his final illness.
31. Cannon (ref. 6), p. 58.
32. Ibid., p. 47.
33. Thackray and Morrell (ref. 8), p. 224.
34. Ibid., p. 234.
35. Ibid., p. 235.
36. Ibid., p. 237.
37. Ibid., p. 236.

38. Ibid., p. 235.
39. The full quotation is: 'In 1833 at Cambridge, Sedgwick as President pressed the case for natural theology, being conveniently joined by Thomas Chalmers, the Evangelical Scottish Presbyterian. No fewer than five authors of Bridgewater Treatises (Whewell, Buckland, Clamers, Prout, and Kirby) were present at the 1833 meeting, thus confirming the close alliance between the British Association and the liberal natural theologians.' (p. 237). The words 'being conveniently joined' might be taken to imply something casual, almost accidental, when in fact the views of Sedgwick and Chalmers were quite similar - and neither should really be slipped in with the liberal Anglicans. Would Chalmers really have agreed that later Bridgewater authors including himself 'saw the task in liberal Anglican terms?' (p. 227) Would he have seen his presence in 1833 as 'confirming the close alliance between the British Association and the liberal natural theologians'?
40. Particularly to John Hedley Brooke amongst those named.
41. I. Lakatos and A. Musgrave, Criticism and the Growth of Knowledge, p. 3.
42. The concentration of the present thesis on matters of theology and philosophy (including scientific methodology) has kept the subject matters manageable if still fairly wide. It must, however, be emphasized that much further interesting work remains to be done exploring the social and political dimensions of Sedgwick and his circle, and the connections between these and the theological and philosophical ones. Restriction of area is expedient to maintain thoroughness but manageability, but always remembering that the actual thinking of a multifaceted person like Sedgwick remains unitary.

## (2) SEDGWICK'S BASIC THEOLOGY

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### 2.1 Church Parties

#### 2.1.1 Introduction

An aim proper to historical research is to see individuals as a part of various movements and currents of thought in their ages and cultures. Sedgwick's theological ideas, therefore, are not to be seen in a vacuum but are rather to be located against a situation of various church parties within the Anglican communion of which he was a member. In looking at church parties, however, we may accept Owen Chadwick's comment: 'Parties are never monolithic. Whenever they are powerful they are numerous, and whenever they are numerous they contain a wide range of opinion.'<sup>1</sup> This diversity within parties often makes it difficult to determine what exactly identifies them and who exactly were associated with them - especially when there is no formal membership. Concepts of parties are, after all, only useful instruments used in analysis, not 'real' entities.

Nevertheless there are two unsatisfactory features of recent historical literature on Sedgwick and church parties. Firstly, though the most common tendency has been to associate him with the 'Broad Church',<sup>2</sup> comment varies wildly, and he has even been called a 'fundamentalist'.<sup>3</sup> Secondly, the concept of the 'Broad Church' is itself suspect. Thus the viewpoint argued in the rest of the present section is that Sedgwick's position was basically Evangelical, and that the term 'Broad Church' as applied to the period of Sedgwick's hey-day has little discernible meaning and confuses rather than aids any analysis.

#### 2.1.2 General Background

A basic familiarity will be assumed in this thesis with the various

terms in common use in the first half of the nineteenth century to describe theological views or parties. Terms like 'Calvinist', 'Arminian', and 'Noetic' are well explained in secondary literature, and there is much material on the rise of the Oxford Movement from the mid 1830's. Some points need, however, to be made, especially about those terms and movements most important to Sedgwick.

The historical origin of the term 'Evangelical' (derived, of course, from 'evangel' or 'good news' of the gospel) is obscure.<sup>4</sup> Simeon noted that in his day (1811) it was still used in reproach.<sup>5</sup> Confusingly, it may be used both of Dissenting groups (excluding the Unitarians<sup>6</sup>) and of a group within Anglicanism. Indeed, it was the same stream of Evangelical revival that flowed into most branches of Dissent which flowed through to Anglican Evangelicalism.<sup>7</sup> What may be called the Wesleyan revival was its immediate source, with influences coming from mainstream Protestantism, Nonconformity (Wesley himself being influenced by the Moravians), and Puritanism. If Wesley and Whitefield epitomise the two theological extremes of the revival, it must be remembered that both were intimate with Henry Venn senior (1727-97), author of The Complete Duty of Man, friend and spiritual guide to Simeon and a founder of the Clapham group.<sup>8</sup> Wesley himself was a universal religious genius, drawing inspiration from the Moravians, William Law, and the Latitudinarian Stillingfleet.<sup>9</sup> Wesley, however, no less than Venn, always remained a loyal Anglican; yet, even at this stage, they diverged about the extent to which mechanisms and structure outside established Anglican ones (and sometimes involving Dissenters) might properly be utilised.<sup>10</sup> Only gradually, however, did these divergences lead the 'Methodists' to be seen as a distinct denomination. Thus on the one hand through the first quarter of the nineteenth century a large group of Wesleyan Methodists regarded themselves as Anglicans<sup>11</sup>, whilst on the other hand Anglican Evangelicals might be called 'Methodists' though

they belonged to no Methodist Society.<sup>12</sup>

Who were the Anglican Evangelicals ? In a movement, as distinct from a sect, this kind of question is difficult. Of a later period Toon wrote: 'No one doubts that Lord Shaftesbury, Edward Bickesteth and Archbishop Sumner were Evangelicals, but other churchmen are less easy to categorise.'<sup>13</sup> There seems, however, a general view that 'the name, the influence, which characterises most deeply and broadly the Evangelicalism of the first third of the century, in the ranks of the Christian ministry, is certainly that of Charles Simeon (1759-1836).'<sup>14</sup> In an often quoted letter Macaulay later wrote: 'As to Simeon, if you knew what his authority and influence were, and how they extend from Cambridge to the most remote corners of England, you would allow that his real sway in the church was far greater than any primate.'<sup>15</sup> Views of Simeon, therefore, and to a lesser extent Sumner (from a slightly later era) and Wesley himself (from an earlier) may be usefully compared with those of Sedgwick. The specific Evangelical theology (which has often been caricatured or misunderstood) is well described by Toon and will not be repeated here.<sup>16</sup>

A term which needs to be distinguished carefully from 'Evangelical' is 'Low Church'. It was coined in the early eighteenth century, describing clergy often sceptical of revelation and said by Balleine to be 'the chief persecutors of the Methodists and early Evangelicals'.<sup>17</sup> In the early nineteenth century, therefore, 'Evangelicals' and 'Low Churchmen' may still be distinguished. With, however, the rise of Tractarianism within the High Church, the term 'Low Church' came around mid century to be applied also to Evangelicals, and less to those whose 'Latitudinarian' sympathy would now cause them to be called 'liberals' or 'Broad Church'.<sup>18</sup> Thus, writing in 1853, Conybeare naturally categorises the Evangelicals as a species of 'Low Church', in distinction to the 'Broad Church' who would have been called 'Low' in a previous generation.

Care also needs to be taken in recognising the relationship between High Church and Tractarianism. The term 'High Church' arose in the late seventeenth century, denoting those who stressed the importance of the episcopacy and the sacraments, and who were correspondingly more antagonistic to Dissenters whom they saw as having rejected important elements of the faith. In the early nineteenth century the term implied one who was (in Chadwick's phrase) 'stiff for the Church of England'. Generally High Churchmen opposed Catholicism, Low Church or Latitudinarianism, and the Evangelicals - though the term was not always used exclusively, and some Evangelicals around 1831 were content to be called 'High Churchmen'.<sup>19</sup> The Tractarian movement which arose from the mid 1830's aimed both at meeting the perceived threat to Establishment from the Dissenters, and at restoring spirituality within the Anglican communion. Its early appeal was therefore both to keen High Churchmen and to Evangelicals. By about 1840 the Evangelical sympathy had largely evaporated, and Newman's infamous Tract 90 in 1841 (arguing that the Articles of the Church of Enagland could be interpreted in a Catholic manner) raised suspicions amongst many High Churchmen. After Newman's own secession from Anglicanism to Rome in 1845, Pusey continued to lead an Anglo-Catholic version of the movement whilst yet another stream of it flowed into Ritualism. Both of the latter could not help but influence and be associated with the High Church.

### 2.1.3 Periodicals

The Christian Observer was indisputably the mouthpiece during the first half of the nineteenth century of the moderate Evangelical Anglican leadership. From 1802 to 1816 it was edited by Zachary Macaulay, a leader in the Clapham Sect. From 1816 until 1850 it was edited by S.C.Wilks, a graduate of the Evangelical stronghold St. Edmunds Hall and a friend of both Simeon and Hannah Moore.<sup>20</sup> The other main Evangelical publication, The Record, first appeared as a weekly in

1828. It was dominated by Alexander Haldane until his death in 1882. A proper perspective on its place and importance is essential for our present theme, since it differed greatly from the Christian Observer and has sometimes been taken as more typical of Evangelicalism. Conybeare, in 1853, put it at the extreme right (Calvinistic) wing of Christianity and the Record did not object to this general placing.<sup>21</sup> But what was the paper's real influence? Chadwick simply wrote that for Evangelicals of the period: 'Of their two journals the Christian Observer practised charity and had few subscribers, the Record acted vituperative partisan and had many subscribers.'<sup>22</sup> This, however, may be misleading. The Record appeared weekly (or even more frequently in its early years) and appealed to a popular readership without analysing in the depth of the Christian Observer. Circulation figures in themselves indicate neither depth of agreement nor the importance of supporters. On the former, leading Evangelical J.W.Cunningham wrote: 'Though many take the paper as a matter of convenience, almost every reader of my acquaintance is loud in his complaints both of the temper and the logic of the leading articles.'<sup>23</sup> Influential Anglican Evangelicals were often highly critical. As early as 1833 the Christian Observer expressed its 'sincere grief' that a potentially good publication 'should have impeded its usefulness by its violent party spirit, its utter lack of good temper, its gross injustice, and its advocacy of whatever is arbitrary and bigoted...'<sup>24</sup> Haldane's piety was never doubted, but his dour Scottish Calvinism was not typical of English Evangelical Anglicanism. Balleine comments: 'Haldane was never a typical Evangelical: his dour tone and unceasing controversy were deeply distasteful to many.'<sup>25</sup> Evangelical leaders disliked its tone. Simeon is reported to have had an 'unconcealed repugnance to the tone and temper of the Record.'<sup>26</sup> Archbishop Sumner declared: 'The conduct of the Record is execrable.'<sup>27</sup> Henry Venn the younger reveals in his



private journals his not infrequent dissatisfaction with its editorial utterances.<sup>28</sup> We must, then, look to the Christian Observer rather than the Record for viewpoints on geologists like Sedgwick from leaders of Evangelical Anglicanism.

Two other publications need mention. During the 1830's the Christian Remembrancer spoke for a more High Church Position. The British Critic was published as a quarterly from 1824. In the early 1830's it was less High than the Remembrancer, eulogising the Dissenter Robert Hall and more positively reviewing Chalmers' Bridgewater Treatise.<sup>29</sup> During 1838 it came under the control of Newman and the Oxford group, and by 1843 the extremity of its viewpoint (in the wake of the storm following Tract 90) had cost it subscriptions of so many High Churchmen that it was forced to cease publication.

## 2.1 Notes

1. O. Chadwick, The Victorian Church, 1, p. 440.
2. Sedgwick was designated a Broad Churchman at least as early as 1940 by Winstanley (Early Victorian Cambridge, p. 94), also by Thackray and Morrell, Speakman and Cosslet (see section (1) n. 12, 13 etc).
3. See Paul H. Barrett, 'The Sedgwick-Darwin Geologic Tour of North Wales', Proceedings of the American Philosophical Society, 1974, p. 146.
4. G.R. Balleine, A History of the Evangelical Party in the Church of England, p. 40, cites some very early instances; the Shorter Oxford Dictionary associates it with the Methodist revival.
5. C. Simeon, Horae Homileticae (Works), 16, Sermon 1833, pp. 40-41.
6. The Unitarians dated as a distinct group in England from the 1770's. Other Dissenters (by now mostly Evangelical) tended (along with Evangelical Anglicans like Sedgwick) to regard them as beyond the pale.
7. See e.g. G. Best in A. Symondson (Ed.) The Victorian Crisis of Faith; Chadwick (ref. 1), p. 5.
8. See C.J. Abbey and J.H. Overton, The English Church in the Eighteenth Century, p. 140; the entry in the Oxford Dictionary of the Christian Church, etc.
9. See e.g. F. Baker, John Wesley and the Church of England, p. 145.
10. On the various other differences, F.K. Brown in Fathers of the Victorians perhaps overstates the case, but see D.N. Samuel (Ed.), Evangelical Succession, ch 5, and also C. Smyth, Simeon and Church Order especially p. 281.
11. Chadwick (ref 1), 1, p. 79.
12. Thus Goreham, opposing Sedgwick for the Professorship, described

Queens as a Methodistical College and himself as a Methodist (Clark & Hughes, 1, p. 157).

13. P. Toon, Evangelical Theology 1833-1856, p. 4.
14. H.G. Moule, Charles Simeon, p. 8 (though others say similarly).
15. G.O. Trevelyan, The Life & Letters of Lord Macaulay, 1, p. 68.
16. Toon (ref. 13) has an excellent summary on page 5.
17. Balleine (ref. 4), p. 165
18. The Shorter Oxford Dictionary (under 'Low Church') makes this point; G.W.E. Russell in A Short History of the Evangelical Movement p. 104 associates a name change with decline and a reaction to Tractarianism.
19. J.S. Reynolds, The Evangelicals at Oxford 1735-1871, p. 3.
20. See the DNB on S.C. Wilks.
21. W.J. Conybeare, 'Church Parties' in Essays Ecclesiastical and Social (5th Edn. 1855). The Record reviewed it in 14th November 1853.
22. Chadwick (ref. 1), 1, p. 448.
23. A. Blomfield, Memoir of C.J. Blomfield, 2, p. 47.
24. Christian Observer, December 1833.
25. Balleine (ref. 4), p. 163.
26. See Brown (ref. 7), p. 129.
27. A.R. Ashwell, Life of Samuel Wilberforce, 2, p. 219.
28. E. Stock, The History of the Church Missionary Society, 4, p. 60.
29. British Critic, 1833, 26, 292-338 and 28, 239-282; this is in contrast to the Christian Remembrancer on both counts which was critical of the Christian Observer for printing Hall's sermons, and of Chalmers as a non-Anglican choice to write a Bridgewater Treatise.

## 2.2 Religious Issues and the Broad Church

### 2.2.1 The Origin of the Term

There is no doubt as to the origin of the term 'Broad Church' to denote a Church party. Though the phrase itself was first used around 1850, its first use (e.g. in an article by Dean Stanley) was to describe the Church of England as a whole.<sup>1</sup> In 1853, however, W.J. Conybeare published his famous article on 'Church Parties' in the Edinburgh Review.<sup>2</sup> His identification of the various parties and their theology is summarised in Table 2.1. As may be seen, he distinguished three main parties: Low, High and Broad, in each of which there were three types: moderate, extreme and stagnant. This was the first time the term 'Broad' was used in this way.

PARTY:	TYPE:	DOCTRINES:
Low/ Evangelical (3,300)	Moderate:	'Universal necessity of conversion' 'Justification by faith' 'Sole authority of Scripture as the rule of faith'  (e.g. Milner, Martyn, Wilberforce, Simeon, Goode & J.B. Sumner)
Low (2,500)	Extreme: (Recordite)	Worthlessness of morality Predestinarian fatalism Verbal inspiration of Scripture Faith = belief that a man is to be justified by faith
Low (700)	Stagnant (Low & Slow)	
High/ Anglican (3,500)	Moderate	Justified by faith, but judged by works Assent to sole supremacy of Scripture, but the Church has authority in controversies of faith Baptismal Regeneration Apostolical Succession  (e.g. early Tractarians Bishop Wilberforce)
High (1,000)	Extreme (Tractarian)	Apostolical Succession paramount Mediatoral Priesthood  (e.g. Phillpotts)
High (2,500)	Stagnant (High & Dry)	
Broad (2,800)	Moderate	Conversion by Grace Justification by Faith Judgement by Works Scripture is the only rule of faith Believe all from Gorham to Bennett to have a place in the C. of E.  (e.g. Hare, Maurice)
(20 ?)	Extreme (Infidels)	
(700)	Stagnant	

Table 2.1

Conybeare's Classification (1853 and 1855) of parties in the Church of England.

Our concern here is not with Conybeare's undoubted inadequacies in his analysis of differences between Evangelicals and High Churchmen - for there were at least recognisable differences in emphasis for these. Our concern is with his newly invented term 'Broad Church'.

### 2.2.2 The meaning of 'Broad Church'

There has always been doubt about how far they constituted a 'party' in any meaningful sense. Even Conybeare himself wrote that they: 'Have so little organisation or mutual concert of any kind that they can scarcely be called a party at all.'<sup>3</sup> R.E.Bartlett in 1878 said that the movement was a tendency rather than a party, and lacked any bond of 'dogmatic agreement' - lacking both literature and leaders in the sense of High and Low Churches.<sup>4</sup> The Shorter Oxford Dictionary says that the term is 'not used in the same manner' as the terms 'High Church' and 'Low Church': 'The Broad Churchmen, so called, not having, like the High and the Low Church, a party organisation, and seldom acting together as a party.' Cornish (1910) emphasizes that they are 'men differing greatly among themselves'.<sup>5</sup> Recent writers have, in theory, recognised this. Sanders admits: 'The Broad Churchmen did not form a compact and homogenous party or school, but frequently differed among themselves almost as much as they differed from others...'<sup>6</sup> Chadwick admitted: 'The term is vague. The group was not a group but scattered individuals working towards the same ends.'<sup>7</sup> Crowther says: 'The Broad Church was not a party, but a set of individuals, many of whom disagreed with each other except in the idea that the authority of the Bible and the Church might be subjected to historical and scientific criticism.'<sup>8</sup> Even Thackray and Morrell admit: 'labels for parties in the Victorian church are notoriously imprecise and should be treated with caution.'<sup>9</sup> But this is a general caution, not specifically related to the

so called 'Broad Church', and they go on elsewhere to call the Conybeare analysis 'the best contemporary discussion' and state of a group including Sedgwick: 'Their theology was that of a distinct party within the Church of England, the 'liberal Anglican' or 'Broad Church party.'<sup>10</sup>

But if most writers before the Cannon-Thackray-Morrell era have been cautious over how far the 'Broad Church' constituted a party with distinctive theology, what of its membership? This seems equally doubtful. Conybeare mentions Hare, Thomas Arnold, F.D.Maurice, Wilson, Dean Dawes, and Dr Jackson. Bartlett gave as its most noteworthy members: Bishop Butler, Whately, Arnold, Maurice and Kingsley. Thirlwall named Hare and Jeremy Taylor as typical.<sup>11</sup> Cornish included Coleridge, Hare, Maurice and Thirlwall, and possibly by implication also Arnold, Whately, Powell, and Sedgwick.<sup>12</sup> More recently, Chadwick names Maurice, Stanley, Jowett and Baden Powell.<sup>13</sup> Cockshut names Arnold and A.Stanley.<sup>14</sup> LeMahieu cites Arnold, Hare, Thirlwall, Maurice and Whately.<sup>15</sup> Crowther gives Arnold, Hare Thirlwall, Maurice, Whately, Powell and possibly Hampden.<sup>16</sup> According to Cannon, its 'recognised leaders after the death of Arnold were Hare, Maurice, Thirlwall, and Arthur Stanley.'<sup>17</sup> The Oxford Dictionary of the Christian Church includes (in various places) Hare, Arnold, Stanley, Whately, Hampden, and possibly Coleridge. Thackray and Morrell include very many names, e.g. Hare, Thirlwall, Herschel, Buckland, Sedgwick, Lloyd, Harcourt, Whewell, Peacock, E.Stanley, Whewell, Arnold, (Henslow ? p.237), and in verbally presented papers Morrell has even included Blomfield. Table 2.2 presents some of these selections, though it has, of course, to be recognised that an omission may not necessarily indicate a fixed opinion

Table 2.2

Table to show different individuals assigned to the  
'Broad Church' by various different commentators.

PERSON:	Commentator:										
	Conybeare	R.E.B.	Cornish	Sanders*	Oxford Dic. of Church	Cockshut	Cannon	Chadwick	Crowther <sup>+</sup>	LeMahieu	Thackray- Morrell
COLERIDGE			✓	✓				?			
HARE	✓		✓	✓	✓		✓		✓	✓	✓
MAURICE	✓	✓	✓	✓			✓	✓	✓	✓	
THIRLWALL											
ARNOLD	✓	✓	✓	✓	✓	✓	✓	?	✓	✓	✓
HAMPDEN				✓	✓				✓		
WHATELY		✓	?	✓	✓				✓	✓	
STANLEY (A)					✓	✓		✓	✓		✓
POWELL			?	✓				✓	✓		✓
SEDGWICK			?								✓

\* NOTE: Sanders adds some qualifications about including them all under one umbrella.

+ NOTE: But Crowther notes that Hampden, Thirlwall and Whately all rejected Essays and Reviews.

that a person was not a 'Broad Churchman'. Top of the poll comes Thomas Arnold. He died in 1842, eleven years before the term was invented, and so had no opportunity to comment on it. In light of his views on the comprehensiveness of the church one can only guess the kind of reaction he might have had to being given a party name. Next come the Cambridge men Hare and Maurice. Hare died in 1855, before the term 'Broad Church' had really become established. Maurice wrote to Isaac Taylor in 1869: 'I do not well know what the Broad Church is, I always took it to be a fiction of Conybeare's.'<sup>18</sup> Maurice went on to suppose that if the term meant anything 'it is a representation, under different modifications, of that creed which is contained in Whately's books...' But Maurice would prefer to let the most extreme Evangelicals revise the prayer book rather than entrust the task to such a group. At least the Evangelicals, like Maurice, had definite views and emphasized relationship with God. But of the Whately group he says they evidence above all others: 'the greatest disease of our time, that we talk about God and about our religion, and do not confess him as a living God...'<sup>19</sup> His son's biography of him speaks of Maurice's dread of the name 'Broad Church',<sup>20</sup> and in 1870 he spoke of his feeling of isolation in the church but of very profound differences from the Liberals: 'we are not a step nearer to each other in 1870 than we were in 1835. They have acquired a new name. They are called 'Broad Churchmen' now, and delight to be called so. But their breadth seems to me narrowness.'<sup>21</sup> It is hardly surprising then, that Masterman has emphasized Maurice's dogmatism<sup>22</sup>, whilst Binns flatly denied that he was a Broad Churchman at all.<sup>23</sup>

After Maurice in the poll of Table 2.2 comes the Cambridge contemporary of Hare and Sedgwick, Connop Thirlwall. He was certainly nearer in spirit to the Noetics, but refused to see



'Broad Church' as a party term:

'Let others interpret it as they will, to me it does not appear an appropriate description of any existing "school", party, or body, held together by a common set of theological tenets. I understand it... as a disposition to recognise and appreciate that which is true and good under all varieties and forms... I have hope and believe that there are numbers who have a rightful claim to it, among those who only profess to belong to one or other of the two great sections of the church.' (1875)<sup>24</sup>

Of the four most commonly asserted members of the Broad Church, then, one died before the term was invented, one died around the time it was first used, one flatly denied membership, whilst the fourth objected to the term being used in the manner of Conybeare (and Cannon and Thackray and Morrell) to mean a party. Perhaps it was a Broad Church deacon who coined the phrase 'curiouser and curiouser'.

Can more light be shed by looking for a common theology ? Since Conybeare invented the term, we may begin by looking at the points which he raised as distinctive:

'The doctrines taught by this party are the same in which both High and Low Church are agreed. The Incarnation and the Atonement, conversion by Grace, and Justification by Faith, Scripture is the only rule of faith; but hence they deduce a conclusion which many Low Churchmen would repudiate, that all who believe the Scripture are members of the household of faith.'<sup>25</sup>

As a distinction this is implausible. Evangelicals too would surely believe that anyone who really believed and acted on the Scripture

would be Christians - but the question centres on whether or not all who claim to believe and follow the Bible, do in actual fact 'really' do so. In any case, Conybeare says only that many, not all Low Churchmen repudiate the idea - so it cannot be used as a distinguishing mark for Broad Churchmen. He then continues:

'With the High Church, they affirm the doctrine of Judgment by Works; and thence infer that salvation depends not upon the ritual but the life; that the fruits of the Spirit are the sole criterion of the Spirit's presence.'<sup>26</sup>

This, however, is a position which Simeon or any moderate Evangelical would have accepted, assuming he is speaking of a criterion to be applied by others.

His next point concerns a Broad Church emphasis on the 'Visible Church; an idea ignored in the teaching of the Evangelicals.' This is contentious, but though it might be applied to Arnold, Hare and Maurice it surely cannot apply to those associated by such as Thackray and Morrell. Conybeare himself 'hedges his bets' by telling us that there are actually two kinds of Broad Church: 'theoretical and anti-theoretical sections.' These cooperate in actual good works, but 'the opinions which we have ascribed to the party are those of its theoretical members; and from these many of the other section would shrink with alarm.' In other words, it is not a real hallmark of the party.

He then goes on to their supposed toleration: 'They will not allow themselves to feel jealousy or suspicion towards any party which professes to fight under the banner of the Church...'<sup>27</sup>

This surely says too much. In a church including viewpoints from Recordite to Tractarian even the most ardent defenders of latitude of thought found someone to be suspicious of - even if only because that someone opposed such latitude. Newman felt Arnold's suspicion,<sup>28</sup> Hare accused a critic (probably Palmer)

of bearing false witness<sup>29</sup>, and so on. Conybeare ascribes the looseness of the Broad Church to its tolerance: 'A common hate is the cement to consolidate a party.'<sup>30</sup> But if we take 'refusal to hate' as a hallmark of the Broad Church<sup>31</sup>, then it would include both moderate Evangelicals (e.g. Bickersteth) and moderate Tractarians (arguably including the Anglican Newman).<sup>32</sup>

On the other hand even Crowther, a believer in the Broad Church concept, admits that in controversy: 'some Broad Churchmen became embittered and as dogmatic as their opponents.'<sup>33</sup> Sedgwick was, in many respects, tolerant of other Christian viewpoints, but both Hare and Sedgwick were certainly outdone by (say) William Wilberforce. His sons in their biography (1838) tried to play down both his Evangelicalism and his connections with Dissenters. But Pollock in a recent biography states: 'Wilberforce built a bridge between warring camps in the Church of England', and cites a letter in which Wilberforce states his wish to see 'a rational and an honest zeal for fundamentals, in place of a hot party spirit.'<sup>34</sup>

Pollock also cites Wilberforce's lament: 'They think I cannot be loyal to the Established Church because I love Dissenters.' and notes his admiration of the spirituality and earnestness of the evangelical Dissenters.<sup>35</sup> Unitarians, however, are excluded from this because Wilberforce deplored their doctrines<sup>36</sup> - though this did not prevent his friendship with the Unitarian William Smith. Wilberforce was willing to aid Bentham on prison reform, though would hardly have approved his religious views.<sup>37</sup>

Another, more recent, biographer points out his connections not only with Dissenters including Unitarians, but that in 1832 he engaged a Catholic tutor for a grandson!<sup>38</sup>

I have cited these instances at length because no one doubts Wilberforce to be one of the leading Evangelicals until his death in 1833. Yet any

attempt to make refusal to hate and friendship with those of other persuasion into a hallmark of a 'Broad Churchman' must include him.

Conybeare totally failed to give any real distinguishing features of the supposed school, and others have similarly failed. E.H. Plumptre, for example, in a preface to Hare's Victory of Faith, wrote of the key as a refusal to accept party character or shibboleths and courage to defend an unpopular but just cause. Yet this, surely, would have applied equally to the Anglican Newman, no less than to Simeon, Wilberforce, both Sumners, Shaftesbury, Pusey or Phillpotts ? In any case, to use non-party membership as a hallmark of a party is a self contradiction.

The term 'Broad Church' as applied anachronistically to the second quarter of the nineteenth century has no ascertainable meaning, and leads only to confusion.

### 2.2.3 Coleridge and his School

Abandoning as misleading the term Broad Church for the period still leaves us to examine the contention of Thackray and Morrell and others (as outlined in my Introduction) that Coleridge and his followers were influential in the thinking of Sedgwick and his circle. The remainder of this chapter will therefore look briefly at some of the ideas of the Coleridgeans in order to determine later whether Sedgwick exhibits anything distinctively Coleridgean.

There is, of course, great difficulty in understanding exactly what Coleridge himself was saying, and scholars both now and then often have disagreed. Nevertheless there seem to have been a number of areas where he and his followers have diverged from Evangelicals:

- (i) the nature of the atonement
- (ii) The nature of Biblical Inspiration
- (iii) The importance of Evidences for Christianity

(iv) The importance of Physico or Natural Theology

(v) Epistemology (reflecting on the nature of Theological knowledge)

Included in the first of these are questions about the Godhead.

Coleridge had gone through a 'Unitarian' phase, from 1796 to 1806 was moving towards a more orthodox Trinitarianism, and was later a vehement anti-Unitarian.<sup>39</sup> Nevertheless, contemporary critics such as Traill<sup>40</sup> and Rigg<sup>41</sup> suspected that Coleridge never progressed much beyond Pantheism or at best Sabellianism - though more modern commentators may have denied this.<sup>42</sup> Rigg's view is especially interesting to us as he wrote from an Arminian (Wesleyan) Evangelical viewpoint around mid century. Turning from God to man, Coleridge had little in common with the optimism of late eighteenth century 'Low Churchmen', being at times overwhelmed by the gravity of original sin.<sup>43</sup> Like Arminian Evangelicals, Coleridge rejected the idea of inherited guilt and saw the fall in individual terms, but unlike them he went further to doubt the importance of the historic fall at all. Guilt itself is minimised, fallenness is reflected in dysfunction, and redemption is seen as making possible 'reintegration of personality.'<sup>44</sup> Coleridge made biting attacks on the idea of 'vicarious satisfaction' of a debt owed to God, calling it a 'gross perversion of the sublime idea of Redemption by the cross,'<sup>45</sup> taking, as Shedd indicates, biblical references to it as metaphorical.<sup>46</sup> All this is in strong contrast to the Evangelicals. It had also implications for the incarnation (was it really necessary at all?<sup>47</sup>), and for faith. Rigg argues strongly on the latter that to Coleridge it means an enlightenment rather than true repentance and knowledge of sin forgiven.<sup>48</sup>

Coleridge rejects the extreme Calvinism of Edwards<sup>49</sup> but also attacks what he calls Arminianism.<sup>50</sup> The free will issue he finally leaves (in fact like Simeon<sup>51</sup>) as a mystery.<sup>52</sup> On hell though some argue that he hinted at universalism,<sup>52</sup> the hint was not clear and Coleridge says:

'God has not promised Heaven finally to all men...'<sup>53</sup>

Coleridge's actual attitudes to the Evangelicals were ambivalent. His favourite works included Luther's Table Talk, Baxter's Life and Southey's The Life of Wesley.<sup>54</sup> His notes in the latter, however, are often critical of Wesley. He distrusted Methodist emotional appeals, thought them to ignore Scripture in making them<sup>55</sup>, and was critical of the emphasis on the need for each individual to be called, converted and chosen with the search for 'signs' that this had happened.<sup>56</sup> His letters make little or no reference to leading Evangelicals of his day. In one Lay Sermon he says '... the Religion of best repute among us holds all the truths of Scripture and all the doctrines of Christianity so very transcendent, or so very easy, as to make study or research either vain or needless.'<sup>57</sup> The Editor takes this as a reference to Wilberforce and his A Practical View..., and Coleridge did say he could 'never feel any sympathy' with Wilberforce.<sup>58</sup> Coleridge and Chalmers were on cordial terms, though Chalmers found him unintelligible, whilst Coleridge in praising one of Chalmers' sermons adds 'Thinking so differently from him upon religion as I do.'<sup>59</sup>

The man who might perhaps seem to illustrate best the supposed links of Coleridge to a Cambridge Network is Julius Hare. Entering Trinity in 1812, a fellow in 1818, in 1832 he went to become minister in Hurstmonceux in Sussex. He was a friend of Sedgwick, even more of Whewell, but would have claimed Luther and Coleridge as the two greatest influences on his theology.<sup>60</sup> Some notes of caution must, however, be sounded. Several sources note his affinities with Evangelicals, even whilst calling him 'Broad Church' or noting more 'liberal' opinions.<sup>61</sup> Most interesting is the near contemporary view of Rigg, who as an Arminian Evangelical sees Hare in a radically different light from both Coleridge and Maurice. Rigg rightly complains about Conybeare's designation 'Broad Church' as including men of 'radically different tendencies in philosophy', and restricts the term to the 'school of

Coleridge' (Hare, Maurice and Kingsley).<sup>62</sup> Yet even then, whilst highly critical of Coleridge, Hare is 'a prince in intellectual wealth' but 'a devout and humble Christian' who has 'left the world to find out its loss.'<sup>63</sup> Rigg writes in puzzlement about Hare. Though Hare claims to follow Coleridge, Rigg can not really see that this is so. Hare's writing 'breathe more of an evangelical spirit' and 'Often, indeed, he all but comes up to the full standard of explicit evangelical orthodoxy'. Though Hare speaks highly of Maurice, Rigg can 'find no trace in all his works of agreement with Mr Maurice's specific views.'<sup>64</sup> Rigg speaks highly of Hare's refutation of Tractarianism and 'complete vindication' of Luther. Rigg finally speculates on the possible influence of Simeon on Hare, citing (though not quoting) the following remarkable passage of Hare:

'By faith Simeon, preaching the word of God in this town through a long life of persevering activity, became the instrument of sending forth zealous preachers of Christ into all parts of the country, and thus contributed, under God's blessing, more than any other man, to that revival of true religion, which has taken place of late years amongst us; and which, we hope and pray, will increase and spread, until in England at least the knowledge of God shall fill the land, as the waters cover the sea.'<sup>65</sup>

Though conceding that Hare's philosophy is transcendental, Rigg finds him orthodox on the Trinity (unlike Coleridge) and on universalism. On the substitutionary atonement (a denial of which is taken as a hallmark of the Coleridgean school) Rigg finds Hare ambiguous.<sup>66</sup>

As we look finally at F.D. Maurice we find a theology which recognises, like the Evangelicals, a problem of sin, but does not begin from this. Rather, Maurice begins from a universal fatherhood of God, with conversion experiences or baptism simply as ways of explicitly

claiming and living out a sonship already a reality.<sup>67</sup> Maurice's universalism relates to this. Rigg, whilst recognising much in Maurice which is 'benevolent wise and beautiful', concludes that he does not preach repentance, faith and atonement in face of future judgement: 'he does not preach Christ's Gospel.'<sup>68</sup>

This excursus into the thinking of Coleridge and the two most often cited as binding him to a supposed Cambridge orientated Broad Church, has been preparatory to a denial that Sedgwick was ever influenced by anything specifically Coleridgean rather than Evangelical. We shall find that Sedgwick's own reactions closely parallel those of Rigg - and for the very good reason that Sedgwick was speaking from a not dissimilar theological position.



## 2.2 Notes

1. See The Shorter Oxford Dictionary.
2. I have used the 5th Edn. (expanded) in Essays Ecclesiastical and Social, 1855.
3. Conybeare (ref. 2), p. 147.
4. Cited in C.R. Sanders, Coleridge and the Broad Church Movement, p. 9.
5. F.W. Cornish, A History of the English Church in the Nineteenth Century, p. 186.
6. Sanders (ref. 4), p. 8.
7. O. Chadwick, The Victorian Church, 1, p. 545.
8. M.A. Crowther, Church Embattled, p. 29.
9. Thackray and Morrell, Gentlemen of Science, p. 226.
10. Ibid.
11. C. Thirlwall, Remains Literary and Theological (1878).
12. Cornish (ref. 5), p. 186-187.
13. Chadwick (ref. 7), 1, p. 545.
14. A.O.J. Cockshut, Religious Controversies of the Nineteenth Century, p. 3.
15. D.L. LeMahieu, The Mind of William Paley, p. 103.
16. Crowther (ref. 8), p. 30.
17. Cannon, Science in Culture, p. 53.
18. Maurice, Life of F.D. Maurice, 2, p. 358.
19. Ibid., 2, p. 359.
20. Ibid., p. 559-607, contains much comment on the 'Broad Church'.
21. Ibid., 1, p. 183-184.
22. C.F. Masterman, Frederick Denison Maurice, p. 172.
23. Binns, Religion in the Victorian Era, p. 145; see also C. Welch, Protestant Thought in the Nineteenth Century, p. 46.

24. Thirlwall, Contemporary Review, October 1875, pp. 703-705.
25. Conybeare (ref. 2), p. 142.
26. Ibid.
27. Ibid., p. 147.
28. A. Mozley, The Life and Correspondence of John Henry Newman, p. 441.
29. Hare: 1849: Letter to English Review.
30. Conybeare (ref. 2), p. 148; M.M. Garland, Cambridge Before Darwin, p. 85, cites Conybeare and accepts this.
31. J.B. Morrell, in verbal presentation, has also spoken of a 'refusal to hate' as a hallmark of the Broad Church.
32. The Anglican Newman spoke of his near agreement with Evangelicals (a letter, 2nd October 1834 in Mozley (ref. 28); of his surprise at finding much he thought his own in Coleridge (in Chronological Notes relating to 1835); at Coleridge's genuine contribution (letter, 2nd September 1835); and of Thomas Arnold's great merits at Rugby (letter to Keble concerning Arnold's death, 12th September 1842).
33. Crowther (ref. 8), p. 19.
34. J. Pollock, Wilberforce, p. 153.
35. Ibid.
36. Ibid., p. 154.
37. Ibid., p. 137 etc.
38. G. Lean, God's Politician, p. 141.
39. See e.g. R.L. Brett, S.T. Coleridge, ch. 8; K. Cooke, Coleridge, ch. 7; J.R. Barth, Coleridge and Christian Doctrine, ch. 1; etc. For later views, see Lay Sermons, p. 249f.
40. H.D. Traill, Coleridge, ch 11.
41. J.H. Rigg, Modern Anglican Theology, p. 12.
42. See T. McFarland, Coleridge and the Pantheist Tradition, p. 223.

43. See e.g. D. Pym, The Religious Thought of S.T. Coleridge, pp. 38, 52.
44. See Ibid., pp. 39, 47; Barth (ref. 39) pp. 115, 120.
45. See Barth (ref. 39), p. 142, also W.A. Shedd, Complete Works of Coleridge, 1, pp. 51-52.
46. See Shedd (ref. 45), also Barth (ref. 39), p. 140, J. Tulloch, Movements of Religious Thought..., p. 442.
47. T. Allsop, Letters, Conversations and Recollections of S.T. Coleridge, p. 47 cites a saying of Coleridge to this effect - taken at face value both by J.H. Muirhead, Coleridge as Philosopher, p. 246, and B.M. Reardon, Religious Thought in the Nineteenth Century, p. 80.
48. Rigg (ref. 41), p. 14; see also Pym (ref. 43), p. 41; faith is fundamentally setting one's human will under the Divine will and so achieving freedom.
49. See e.g. Pym (ref. 43), p. 41.
50. See J.D. Boulger, Coleridge as a Religious Thinker, pp. 55, 177.
51. See Pollard and Hennell, Charles Simeon..., p. 32, etc.
52. Barth (Ref. 39), p. 153, quotes Coleridge's notebooks to show this. Coleridge also refers to the issue in Southey's Life of Wesley (e.g. 1, pp. 260, 270), and calls the Church of England Article on Predestination 'a model of good sense and theological discretion.'
53. Barth (ref. 39), p. 191.
54. For Luther see e.g. Pym (ref. 43), p. 23. Later editions of Southey carried Coleridge's comment that he read it more than any other, and in sickness or languor resorted either to the Life of Wesley or Baxter's Life as to old friends.
55. Boulger (ref. 50), p. 48 cites this.
56. See Barth (ref. 39), p. 145.

57. Coleridge, Lay Sermons, p. 194; on p. 200 (in contrast to Sedgwick and the Evangelicals) he criticises the Bible Society for distributing the Scriptures without commentary.
58. E.L.Griggs, The Collected Letters of Samuel Taylor Coleridge, 6, p. 734. Allsop (ref. 47), p. 38, says he suspected Wilberforce's interest in the slaves to be for his own soul.
59. See Griggs (ref. 58), 6, p. 839, for a visit in 1830, R.W. Armour and R.F. Howes, Coleridge the Talker, p. 17, p. 127 for an earlier visit and Chalmers' reaction, p. 343 for Coleridge's. Also W. Hanna, Memoirs of the Life and Writings of Thomas Chalmers, 3, p. 167-168.
95. Hare defended Luther in a note to his Mission of the Comforter in 1846, and a Vindication of Luther was published in 1854. For his expressions of debt to Coleridge see Sanders (ref. 4), ch. 5.
61. E.g. Binns (ref. 23) he was 'in many respects sympathetic to the Evangelicals' (p. 143); 11th Edn. Britannica, he belonged to the 'Broad Church', 'though some of his opinions approach very closely to those of the Evangelical Arminian school'; Reardon (ref. 47) 'his personal sympathies lay with the Evangelicals' though differing on some issues (p. 160).
62. Rigg (ref. 41), p. 31.
63. Ibid., p. 34
64. Ibid.
65. Hare, Victory of Faith, p. 141.
66. Rigg (ref. 41), pp. 89, 93, etc.
67. Chadwick grossly oversimplifies Maurice's relationship to Evangelicals. For his distinctive approach, e.g., Maurice's Essays, pp. 200-201 or secondary literature.
68. Rigg (ref. 41), p. 113.

## 2.3 Sedgwick's Religion

### 2.3.1 Sedgwick's Background

We have already noted the difficulties of identifying who are members of a party within a church (rather than members of a sect which has some kind of official membership). Sedgwick did not like party labels, and did not declare himself in such terms. This means we need to look at various features of his religious life and beliefs in order to determine what affinities he had with the various church parties of his day. We begin with his religious background.

A person's religious upbringing and background are always important in understanding his or her viewpoint. In Sedgwick's case it is all the more important since throughout his long life (and unlike, say, Connop Thirlwall) he showed little if any change in his basic religious position. He always spoke with greatest respect and love for both father and family, and the inhabitants of Dent. Clark and Hughes point out that this is, moreover, to be seen in the light of his father's living until Sedgwick was forty three, and implies a judgement made in adulthood rather than a vague boyhood memory.<sup>1</sup> What, then, can we know about Sedgwick's father and his religion ?

Richard Sedgwick had attended what was then called Catherine Hall (later St Catherine's College) Cambridge, from 1756-60. This means he graduated three years before Paley (though not from the same college). In 1785 when Adam Sedgwick was born, his father had been vicar of Dent for twenty years, and continued in this office until 1822. He died in 1828, when Adam was 43. The Parish records for the period are uninformative<sup>2</sup>, and for Richard Sedgwick's views on religion we have largely to rely on Adam.

Adam says of his father's parishioners:

'They loved my father, because by birth he was one of themselves, and because of his kindness and purity of life... The influence he had over the minds of his flock rested on his humble teaching of Gospel truth; on the cheerful simplicity of his life; and on his readiness at every turn and difficulty, to be in true Christian love an adviser and a peacemaker.'<sup>3</sup>

All Adam's references suggest a man of simple personal piety, pure but unselfrighteous. He was not legalistic, and did not try to stop the ancient local custom of playing football on Sunday afternoons:

"he thought the contest, if carried on in goodwill, tended to health and cheerfulness... He dreaded too, the acts of intemperance and drunkenness which might arise out of the sudden suppression of a generous and healthy exercise in the open field.'<sup>4</sup>

Charles Simeon himself<sup>5</sup> showed a not dissimilar pragmatic approach to the 'Lord's Day', though he was conscious that others differed. Later attitudes, both of many Evangelicals and of High Churchmen, hardened. Sedgwick notes and laments the 'greater severity of religious rule' at the end of his life.<sup>6</sup> He recognises that a 'formalist or stern high churchman' might blame his father for once refereeing a football match when called upon as peacemaker.<sup>7</sup> Sedgwick often refers to his father's character.

In one letter he says:

'He was a good old man who believed as firmly in the truth of religion as if he saw his Saviour and the home of the blessed with his own eyes. At the same time he was a

cheerful happy man...'<sup>8</sup>

In the same letter he refers to the piety of his sister Margaret, who for the last part of her life was 'as near Christian perfection as is permitted anyone to reach in the trials of this world.' He speaks also of the faith of his brother, picturing his whole family as one of a simple piety - though not without learning.

Of his father's beliefs we may glean little, but there are a couple of important pointers. The first comes in the following words of Adam's:

'Some years before I ever saw the light there was an unexpected contest for the county of York. Mr. Wilberforce, a young man of bright presence and great eloquence, was then named as a candidate; and he had even then become famous as an enthusiastic advocate for the abolition of the slave trade. This fact set every chord of my father's heart in motion.'<sup>9</sup>

Sedgwick was writing this in 1868, and may be forgiven for some lapses of memory. Wilberforce first stood in Yorkshire in 1784, the year before Adam Sedgwick was born. At that time he was renowned neither for his Evangelical views nor for anti-slavery sentiments. By the second election in 1790 he was known for both, and presumably this must be the election to which Adam refers.<sup>10</sup> It may be that there was, as Speakman has suggested, some local influence on Richard Sedgwick, but it is clear that the main opposition to the slave trade came from those of Evangelical persuasion - with John Wesley himself as one of the first to take it up.<sup>11</sup>

In the later eighteenth and early nineteenth centuries serious minded clergymen and Christian laymen would hold family prayers. Smyth explains how many Evangelical clergy would throw open their domestic worship one or more evenings in the week to those of their parishioners who cared to attend.<sup>12</sup> Isaac Milner, for example, was 'always ready and willing to expound any passage concerning which he might be requested to give his thoughts' though he had his favourite passages.<sup>13</sup> High Churchmen might also keep family worship, though stipulated that it should be liturgical and from the Prayer Book, 'whereas the Evangelicals were better satisfied with extempore prayer and exposition or with the use of any edifying book.'<sup>14</sup>

So we find Adam Sedgwick writing of his father:

'There was often at the old Parsonage, on a Sunday evening, a small tea-party for those whose homes were distant from the Church; and, later in the evening, my Father read to a small assembled circle from some serious book (it might be an extract from one of Bishop Wilson's sermons); and the little service ended with a short family prayer...'<sup>15</sup>

Sedgwick's father, from this report, clearly followed the Evangelical practice, and Adam himself in later years followed Isaac Milner's practice in asking his assembled household (in his case of servants rather than wife and family) if there were a passage they would like expounded.<sup>16</sup> The particular 'edifying book' which Adam mentions his father using is very interesting - giving us the only hint we have of his father's theological views. So what do we know of Wilson?

Thomas Wilson was Bishop of Sodor and Man from 1696 to 1755. Various sources are available to us showing his life



and work.<sup>17</sup> The system of church government in the Islands was a strong one, more like the Moravian communities than the mainland Established Church, but Wilson administered it so well that he was idolised by the community. He stood up for the church rights against the state, and for a time was imprisoned by the Governor of the Island. He could not, of course, be classed as an 'Evangelical', for the term had not been invented, and his ministry essentially predates Wesley so he could not be called a Methodist either. Russell classes him as one of the 'more devout adherents of the traditional theology' (in which he also includes Porteous), and sees such theology as one of three streams which 'came together to make the river of the Evangelical Movement.'<sup>18</sup> He was, in the words of his biographer, 'a great friend of toleration', Catholics attended his sermons and prayers, Dissenters came even to the Communion, Quakers esteemed him, and the Moravians made him the equivalent of a Bishop in their system.. It was, in fact, 'only against opinions suspected of a rationalising tendency that his wide hearted charity was closed.'<sup>19</sup> Wilson's relationships with Dissenters and Moravians are very similar to Wesley's. So little, however, was he suspected of 'Latitudinarianism' (or 'rationalising tendency') that even John Keble wrote an appreciative biography of him.<sup>20</sup> Wilson's openness to other Christian denominations was based on awareness of commonality in essentials of the Gospel, not on vagueness.

Since Adam Sedgwick apparently often heard Wilson's sermons in family worship, they are worth close examination. They reveal a man not a Calvinist, but with an Arminian theology similar to (say) Wesley or William Wilberforce.<sup>21</sup> Thus e.g. we read:

'HE THAT BELIEVETH NOT SHALL BE DAMNED. These are not ours,

but the very words of the Son of God. You will say, perhaps, all Christians do believe the Gospel. Would to God we all did believe as we ought to do! But to believe is not enough to justify us before God, unless our faith oblige us to live as becomes Christians. To love, to fear, and to glorify God; to be sober, chaste and temperate; to be just and charitable to men; and, after all, which is the perfection of saving faith, to confess before God, that, when we have done all these things, it is all owing to the mere mercy of God, to his grace, his assistance, and to the merits of Jesus Christ and his intercession, that God will accept and reward our faith, and our poor endeavours to please Him...<sup>22</sup>

Wilson continues to contrast the 'faith of too many pretended Christians, who know the Gospel but live as if not one word of it were true' and adds: 'to awaken and terrify such unhappy sinners as hold the truth in unrighteousness, the Lord Jesus Himself hath given us the very sentence that shall be passed upon such, when He shall come to judge the world in justice: "Go, ye cursed, into everlasting fire."'

Wilson speaks of the invitation of Christ to 'come unto me';

'All, therefore, who will effectually close with this gracious invitation of Jesus Christ, must receive Him as our Lord, to govern us by his Gospel- as our only mediator, to prevail with God for our pardon- and as our only Redeemer, who laid down his life for us.'<sup>23</sup>

Wilson specifically rejects the Calvinist doctrine of election<sup>24</sup>, but accepts very clearly the substitutionary atonement of Jesus as Saviour. At the same time he emphasises holiness of life,

much as Wesley was later to do.<sup>25</sup>

Wilson's little work on The Lord's Supper emphasises the value of the Bible<sup>25</sup>, the fall of man, the substitutionary atonement then made by Jesus, the need for repentance, etc. The story is well known how, in 1779, Charles Simeon first understood the doctrine of the substitutionary atonement of Jesus (in the picture of Jewish sacrifice) through reading this short treatise of Wilson's - leading to Simeon's conversion without involvement of any living counsellor.<sup>26</sup> Moule suggests that the passage Simeon was reading might have been the following:

'As an Israelite, under the law, being obliged to lay his hand upon the head of the sacrifice, confessing his sins, and laying them, as it were, upon that creature, - as he did easily understand that this was to show him that death was the due reward of sin; that this ought to humble him before God, and to give him the greatest abhorrence of sin, which could not be pardoned but by the loss of life of an innocent creature: - as this was plain to the meanest Israelite, even so the most unlearned Christian, when he considers that our Lord Jesus Christ became a Sacrifice for us, and that on Him all our sins were laid, on Him who knew no sin; - he will easily understand how sad our condition was which required such a Sacrifice: - that this therefore ought to humble us, - to lead us to repentance, - to make us fearful of offending God, - and to abhor those sins which cost Jesus Christ his life before God could be prevailed upon to pardon them...'<sup>27</sup>

This is in fact a very strong statement indeed of the doctrine both of substitutionary atonement and of propitiatory

sacrifice. The Coleridgeans, and especially Maurice, would have surely rejected it - though it was evidently the basis of Simeon's conversion and was accepted by all mainstream Evangelical theology. Simeon's own Evangelical Churchmanship (and some said he was too much of a Churchman) is his own, but is much in the tradition of Wilson.

We must here remind ourselves that in this period there was little Evangelical religion of any kind at Cambridge. Simeon fastened on to one of the few writers in anything like that tradition who were available in the University circles. It is the more remarkable that Richard Sedgwick seems (whether also at Cambridge, or later, we do not know) to have aligned himself with the same tradition. And it was this tradition in which Adam Sedgwick was raised. Bishop Wilson's own emphasis on the Bible may well have been reflected in Adam Sedgwick's home. Richard Sedgwick evidently, like most Evangelicals, practised extempore prayer, for Adam records a story of a parishioner overhearing an apparent conversation which turned out to be Richard Sedgwick speaking in prayer to God.<sup>28</sup> Adam, like his father, later prayed extempore, fervently, and aloud in private devotion.<sup>29</sup>

Concerning relationships with Dissenters, Adam wrote that in the early days:

'there were no Dissenters in Dent, excepting the small well ordered Society of Christian Friends... but among them there was no feeling of bitterness and no outward conduct that interfered with the soft current of Christian love.'<sup>30</sup>

Sedgwick's father was on good terms with the Quakers, and we have already noted how he mobilised them to vote for William

Wilberforce. One of his father's particular friends was the Quaker, Robert Foster:

'Sometimes, but rarely, he and my father had discussions at the vicarage on subjects of religious ordinances... They agreed in many of the great essentials of Christian truth; and they agreed that the end of all religious ordinances was to bring the heart - the fountainhead of all true religious emotion - into conformity, both in thought and in outward act, with the revealed will of God.'

When aged 16-19 and at Sedbergh School, Adam boarded with a relative of Foster, 'a kind Quaker family'.<sup>31</sup> Sedgwick always retained his affection for the Quakers (who were strong in his home area), not because of any 'latitudinarian' vagueness of doctrine, but because both his training (under the tradition of Wilson), and his father's example had taught him to appreciate their simple piety.

Adam's own education continued with tuition from John Dawson: 'a firm believer and a good practical Christian of the old school.'<sup>32</sup> Dawson was known as 'a devout Christian of the Established Church.'<sup>33</sup> He was also known as a brilliant mathematician, though choosing to remain in his life as a doctor.

During the nineteenth century Wesleyan Methodism rose in importance in Dent, and in 1868 Sedgwick wrote:

'if they have shown the light of Gospel truth to houses in darkness, and if they have given the hopes of heaven to some who were not reached by the more formal teaching of the Church of England, this is to me a matter of joy: and in making this acknowledgement I believe I am echoing a sentiment which had a former issue in my dear old father's heart.'<sup>34</sup>

In 1832 Adam went to Dent for the commencement of building of the (Church of England) Cowgill Chapel, of which he was a trustee:

'Men of many shades of opinion threw aside their differences that day, and met together in a true spirit of brotherly love. Churchmen, descendants of the early followers of George Fox, Wesleyans of two divisions, and Independents, all appeared that day to be of one mind.'<sup>35</sup>

He records that many of Dissenting opinion had actually contributed generously to the building of the Church of England Chapel in a needy area.

Sedgwick's family background was one which was certainly far closer to Evangelical than to anything else, both in general temper and in theology. It was also one which was tolerant of Dissenters, not because of lack of conviction but because they were seen (as many Anglican Evangelicals saw them) as true Christian brothers in Christ.

### 2.3.2 Sedgwick's Personal Piety

There can be little doubt of Sedgwick's own personal piety and of the genuine qualities of his religious life. Writing to a niece on the death of a friend at Scalby he says:

'Oh! that I had St Paul's faith and could look on death and speak of it as he did! Oh that I had the quiet placid confiding faith which my dear old Father had for many years before he died! I only have it not because God has not given me the grace to deserve it. For Christian faith is God's gift, and is of God's grace, and no living man ever had this grace by counting up his own poor deeds, but rather by asking for it in

repentant humility and in the heartfelt words of the publican- 'God be merciful to me a sinner!'<sup>36</sup>

In a brief unpublished autobiographical sketch Sedgwick spoke of his religious attitude:

'I am convinced that joy and not gloom should be the normal condition of a Christian's life. He has so much to make him joyous to remain long depressed even by real sorrows. And to some natures God has kindly given such elasticity of spirit that they cannot refrain from merriment when others would only see what is serious and sad.'<sup>37</sup>

No one can be immersed in Sedgwick literature and memorabilia without feeling the man's great sense of fun (notwithstanding his undoubted hypochondria which appears in many of his published and unpublished letters). But any stereotyped picture of an Evangelical or 'serious Christian' as a gloomy kind of person is misguided. Milner of Queens was known for his large appetite and for the fact that his: 'public dinners were very merry ones, but the private ones were quite uproarious.'<sup>38</sup> Mme De Stael pronounced William Wilberforce the wittiest man in England, and his asides in the House of Commons could have his near neighbour in stitches of laughter.<sup>39</sup> The Tractarian spirit was against this, and Tractarian Oxford contrasts with the Evangelicals. But whilst Sedgwick's natural gaiety of spirits would have made him a poor Tractarian, it was not unusual amongst Evangelicals. Moreover Sedgwick could also show a very serious side, a consciousness of sin, as when he writes to his Evangelical friend C.N.Wodehouse. The latter, as a fellow Canon at Norwich in 1837, had written to Sedgwick saying that he had heard

criticisms of Sedgwick's sermons , and wondering whether Sedgwick might 'give us some good, well considered Theology from the Prebendal Chair',<sup>40</sup> Sedgwick replied:

'My conscience tells me, in language my soul cannot misinterpret, that in the hourly conduct of my thoughts, and in the daily actions of my life I have not only much to repent of, but that which ought to sink me to the earth and fill me with humiliation and shame. I am now, at least, writing seriously ...'<sup>41</sup>

But he exclaims that he cannot 'tickle people's ears with fine sermons'. Indeed, reports given of his style by witnesses speak of his great feeling in reading Scripture, his eloquence in extemporary expansion on a theme, but not of his ability to write methodical sermons.

He could show simple pastoral comfort, as when he wrote about the death of his friend Armstrong, to the relative (Miss Blamire) who had informed him:

'May we learn, thro' God's grace, to say in our hearts, even in our hours of bitterness and sorrow, Not my will but thine be done. May God, my dear friend, be now your comforter- and enable you even in your present sorrow to feel the strength and comfort of Christian hope .... And may my God and Redeemer enable me to put my house in order before I die.'<sup>42</sup>

In that same year he wrote to Murchison, from whom he had long been estranged, a sincere letter of sorrow about the death of Mrs. Murchison:

'May God teach you to bear your sorrow like a man. Of



this I have no fear; but more than this, may his grace be given you to bear it like a Christian. This sustaining power is his precious gift, and it must be humbly sought for... by prostration of heart whilst under God's afflicting hand. May he give you the comfort of Christian hope; compared with it all other comfort vanishes into mid-air ...<sup>43</sup>

As a further instance one might note an incident in 1856. Sedgwick stood, silent and deeply moved, at the bedside of the old geologist Jonathan Otley - now speechless from paralysis but quite aware of him. For a while he held the sick man's hand, and then, bursting into tears, knelt crying out: 'Jonathan, I'll pray with you.'<sup>44</sup> This simple reaction of heartfelt piety is more striking as letters in the Cumbria County Archives in Kendal show that Otley wrote to Sedgwick only three times between 1847 and January 1854 - they were not bosom confidants. Sedgwick's manner and approach contrasts with the cold detachment of, say, Thirlwall, or the awkward pastoral manner attributed to Hare.

Sedgwick's Christian faith, then, was not a purely formal or intellectual affair. It was a real force in his life, and his piety and prayerful life are attested by his own generation.<sup>45</sup>

### 2.3.3 Sedgwick's Theology

One of the fundamental aims of this thesis is to understand the metaphysical and religious standpoint of Sedgwick, and to establish the kind of stream or tradition into which it fitted. Before we look at any particular comments which he made upon

various church parties, it may be useful to consider his own theological views on a number of issues. His views on issues like church tradition will be clear shortly when we consider comments on church parties, but in the previous chapter we distinguished five areas in which the Coleridgeans had views distinguishing them from mainstream moderate Evangelicals (particularly Arminian Evangelicals). These

were:

- (i) The Atonement
- (ii) Biblical Inspiration
- (iii) Christian Evidences
- (iv) Natural (particularly physico) Theology
- (v) Epistemology (reflecting on Theological Knowledge)

Numbers (ii) to (v) are important enough to the specific issue of religion and science to be treated separately in future sections. But at this point it will be useful to consider point (i), the atonement. Though not directly connected with the science-religion question it was certainly the issue which Evangelicals themselves would have regarded as the most important, and was one of two on which the 'liberals' of various hues were most suspected.

Sedgwick's theology was Arminian in the tradition of Wilson, and in his Discourse he attacked an extreme Calvinist view of total depravity implying that natural man had lost all real moral sense. The first edition of the Discourse did not make it entirely clear whom Sedgwick was attacking, and the vituperative Henry Cole used the passage to accuse Sedgwick of setting up natural in place of revealed religion. The charge was absurd, and Sedgwick flatly denied it in the fifth edition of his Discourse.<sup>47</sup> Sedgwick was, however, concerned that however absurd Cole's charges, his own words as misquoted by Cole had been

misunderstood by more respected and more friendly commentators.<sup>48</sup>

So Sedgwick, after seeking consultation on the note with the leading Evangelical Carus (who was Simeon's chosen successor)<sup>49</sup>, set out clearly his theology, especially on the atonement:

'Let me endeavour, so far as I am able within the narrow limits of this note, to explain my words, which have been misunderstood and strangely tortured from their meaning. The following are among the great fundamental doctrines of our religion - That "man is very far gone from original righteousness, and is of his own nature inclined to evil" - that our Maker is pure and holy, and requires from us nothing less than perfect obedience - perfect both in will and deed. - That every son of man is, therefore, guilty before God, and under sentence of condemnation - That a remedy has been provided for us in the person of Jesus Christ- who not only showed in himself an example of perfect purity and holiness, but opened for us a way to future happiness, by the voluntary sacrifice of himself - thereby satisfying the severe attribute of God's justice, and blotting out the written sentence of our condemnation . - That a faithful acceptance of this doctrine is the principle and foundation of our forgiveness, and by inherent moral necessity fills the heart with thankfulness and love. - That a full perception of these doctrines is not enough. - That after we are forgiven and restored to our Maker's favour, infirmity and corruption still clings to us. - That we require, after admission to the covenant of mercy, through our whole lives continual support

and renovation, - to be maintained only by communion with God and the sustaining grace of his Holy Spirit. - That in this way God may dwell in our hearts as our Maker, our Redeemer, and our Sanctifier. - That from first to last this scheme of salvation is the free gift of God: not purchased by our own works, or claimed by us on any score of self-sustained inherent personal merit of our own. - The proper effects of these great doctrines, when faithfully received in the heart, are an enlarged charity - a purity of life grounded on the highest sanctions - and a ready acknowledgement of the goodness and providence of God in all his dealings with us. - The film which dims our vision falls away - the passions which desolate the world become obedient to law and order - we see in everything around us the power and benevolence of our Maker - all the higher faculties, moral and intellectual, receive the noblest exaltation; and now at length subservient to the true purposes of our being, become "a rich storehouse for the glory of the Creator and the relief of man's estate."<sup>50</sup>

This has been quoted at length because it is a key passage, amply corroborating the theological position argued for Sedgwick in this thesis. Sedgwick did not, of course, mean to imply that a purely intellectual acceptance of the doctrine of atonement was enough to admit a person to the covenant. Elsewhere he makes it abundantly clear that a 'lively' faith is to be distinguished from a faith which is purely intellectual and which is 'a mockery and a snare'.<sup>51</sup> If his words lack the clarity of a Wesley or a Simeon in calling to repentance, we must remember that Sedgwick took for granted that he was speaking to

instructed Christian men<sup>52</sup>, and in any case there are few Wesleys or Simeons in the Evangelicals of a generation. But we may note two aspects clearly Evangelical and unlike Coleridge or Maurice. Firstly, the atonement involves the 'satisfying of God's justice'. Secondly, personal response is needed ('faithful acceptance' - i.e. an acceptance with implications) to partake of forgiveness.

For those in the Covenant, Sedgwick emphasizes the need for a holy life, lived through the 'new power of moral obedience' given by God.<sup>53</sup> As for ordinances, Sedgwick gives a warning:

'... we are no true children of our Lord and Master -  
we are no part of his flock - if we honour him not by  
the outward forms of allegiance he has himself enjoined...  
- by acts of public devotion - by the earnest petitions  
of private prayer...'<sup>54</sup>

His emphasis on habits, particularly the habits of prayer, were again a part of an Arminian Evangelical emphasis on holiness.

In Sedgwick's theology, then, of atonement and sanctification, we find a basic Arminian Evangelicalism, in the Wilson tradition. On the key points where Coleridge or Maurice would have diverged from this tradition Sedgwick emphatically does not, and to associate him in a group with such Coleridgeans (under the title 'Broad Church' or anything else) is quite misleading.

#### 2.3.4 Sedgwick's Friends and Associates

I have argued at length for the inappropriateness of the term 'Broad Church', and noted that many even who accept it sound notes of caution. Cannon, however, abandons all such caution, and it is interesting to see why exactly he does so. He states:

'Conybeare noted that the Broad Church was not a compact body, had no newspaper or magazine or other rallying-point and in contemporary terms was hardly a "party" at all.

These kinds of facts have led Owen Chadwick, in his recent Victorian Church, to overlook the kind of approach used in this essay and state: "The group was not a group but scattered individuals working toward similar ends."

Chadwick is handicapped by the tendency of British scholars to look primarily at Oxford men as the central figures in intellectual movements... By looking at the prominence of churchmen in the Church itself, and then seeing how they were interconnected by personal, family, and Cambridge links, we can see that the leadership of the Broad Church was an actually existing group. Its background has now been sketched, for the recognised leaders after the death of Arnold were Hare, Maurice, Thirlwall and Arthur Stanley.<sup>55</sup>

We have seen how Cannon's picture of an actual group with recognised theological leaders and 'theological' and 'scientific' nodes is unsupportable. Perhaps (to rephrase his criticism of Chadwick) he is handicapped by the tendency of American scholars to misunderstand the nature of personal, family and friendship links formed in the British University system and Anglican Communion. We must, in fact, be careful to distinguish friendship or even admiration from agreement. There are obvious contemporary examples both of admiration<sup>56</sup> and of friendship or family ties<sup>57</sup> without agreement.

We must, also, beware of selectivity. Thus Whewell was certainly close to Hare and sought his advice. On the other hand, another member of Sedgwick's circle, Romilly, seems in his Diary

to indicate that he stood in an analogous relationship with the Evangelical Carus (like Hare with Whewell, a mutual friend of Sedgwick's). Cannon seems almost embarrassed that in a count of the frequency of names in Romilly's Diary Carus comes out fifth - lamely adding 'but several of these entries are notices of Carus sermons...'<sup>58</sup> Yet Romilly's high place in Sedgwick's circle is undoubted, and on his death Sedgwick wrote to Whewell of our 'old and beloved friend' adding: 'he was upheld by Christian faith and he led a life of true Christian love. About his reception before the face of his God and Saviour there can be no doubt with those who knew him and loved him best.'<sup>59</sup>

We find, in fact, that Sedgwick's various circles of friends include both Evangelicals and those called 'Liberal', but few High Churchmen. Sedgwick does refer to his friend Ainger as 'too much of a high churchman for me.'<sup>60</sup> Ainger, however, was an old school friend of Sedgwick's, who preceded him to Cambridge by one year, and was really more of a personal friend of Sedgwick than one of his circle. Sedgwick was prepared, though, to twit his friend on his prejudices, and in urging Ainger's vote for Palmerston adds: 'Goulburn is the idol of the Saints, a prime favourite of Simeon's, and a subscriber to missionary societies. Moreover he squints...'<sup>61</sup> Sedgwick himself was an admirer of Simeon and a later supporter of the Evangelical missionary David Livingstone (whose work took over where Fowell Buxton's had stopped). But a High Churchman might object to all these things. Besides, Sedgwick had a great sense of humour - and we need no more suppose his own objection to Goulbourn to be his Evangelicalism or mission activities than that the man squinted.

A genuine member of Sedgwick's circle was George Pryme. Pryme also came from Sedbergh, and had studied under Dawson (though five

years Sedgwick's senior). In 1863 Sedgwick wrote to him:

'You and I, my dear Pryme, have not long to live in this world...

I believe that you have the Hope given us through Faith in the power and love of our Redeemer... You are one of my oldest friends, and I have generally agreed with you in opinion: and even in points in which we differed I always honoured you as a man of principle.'<sup>62</sup>

Pryme had gone to school under the arch-Evangelical Joseph Milner, and

says: 'I was convinced of the truth of his views, and have ever since adhered to them. He was, in fact, the Simeonite of that part of the country...'<sup>63</sup>

Recording later a visit from Simeon

Pryme writes:

'I now attended Trinity Church, in which parish my house was situate. Mr Simeon, the vicar, called upon us, and sometimes invited us to his rooms in King's College. He was a celebrated extempore preacher, and was founder and head of the Evangelical party at Cambridge. I entertained similar views on some points of doctrine which I had learned from Dr Milner, but I ventured to differ from him as to the impropriety of theatrical entertainments and card-playing... He candidly argued the matter with me; I maintained them to be objectionable only in their abuse...'<sup>64</sup>

It is, then, notable that Sedgwick expresses not merely friendship but general agreement with such a man.

The Evangelical Carus has already been mentioned. He writes:

'I had the privilege of enjoying the friendship of Professor Sedgwick (our families having been long known to each other) from the commencement of my residence in Trinity in 1823.'<sup>65</sup>

Carus was actually present at the delivery of the famous sermon



forming the basis of the Discourse. From Carus' article we learn that he visited the Dent vicarage and knew Sedgwick's brother well. It may also be in a family context that Sedgwick speaks of the 'honoured personal friendship of William Wilberforce and Clarkson',<sup>66</sup> for here there was not a Cambridge connection. In any event, we find the Cambridge circle with Evangelical connections (even in days when Cambridge known Evangelicals were few), as well as with liberals such as Thirlwall.

Turning now to Sedgwick's Norwich connections, we may note Thackray and Morrell's mention of Sedgwick's friendships there with the Dissenters and the Stanleys.<sup>67</sup> In fact, however, the Norwich into which Sedgwick came in 1835 had as Bishop the aged Bathurst - the man who first ordained Sedgwick in 1817 and one of the few Bishops with Evangelical sympathies, who supported both Bible Society and CMS in Norwich.<sup>68</sup> Sedgwick was friends with many at Norwich but the Quaker mentioned as his 'particular delight' was J.J. Gurney.<sup>69</sup> Gurney was the brother of Mrs Fowell Buxton, and was a particular friend of both Wilberforce<sup>69</sup> and Simeon.<sup>70</sup> Gurney's association with the Evangelical stream of Quakerism, and his closeness with the Clapham group are reflected in his Memoirs.<sup>71</sup> Thackray and Morrell, who see the alliance between various denominations in the BAAS as founded on natural theology, assert that 'the liberal Anglicans found strong allies in those sections of the church least given to doctrinal formulas.'<sup>72</sup> This is supposedly true of Quakers, but in fact Gurney's Memoir contains an appendix giving a Declaration of Faith. Sedgwick's feelings of fellowship with such were not based on vague feelings of a God of nature but (as for any other Evangelical) on the basis of Christ's redemption. Sedgwick's 'liberality' was of the same kind as Wilberforce's, noted on p. 28, though Sedgwick was less magnanimous to Unitarians. On

one occasion Sedgwick dined with Evangelical Pryme and there met:

'a gentleman who belonged to a celebrated Unitarian family. In the evening he ventured on a controversial excursion, little knowing who was listening. Sedgwick listened for a while and then broke out with great vehemence:

"Sir! rather than attack and mutilate the Scriptures as your Unitarian friends do, I would prefer to disbelieve the whole Book of Revelation as an Inspired Work, and to put myself at once on my moral conscience as a guide for life and conduct!"<sup>73</sup>

A fellow Canon at Norwich with Sedgwick was C.N. Wodehouse.

Around 1840 he began to argue that subscription to the 39 Articles of the Church of England was a disgrace since so many clergy disbelieved parts of them. He wanted to see subscription replaced by assent to the three creeds, and wrote 'setting forth certain passages in the Liturgy, which, in his opinion, required alteration from an evangelical point of view.'<sup>74</sup> In 1847 Bishop Stanley wished to offer Wodehouse an Archdeaconry, which refuelled the controversy during which Wodehouse (whilst writing in his defence) had some doubts as to the propriety of taking office.<sup>75</sup> Sedgwick's correspondence with him (some of which is quoted in the next part 2.3.5) shows close sympathy and understanding.

Not long before we find Wodehouse writing to urge Sedgwick for some theology, Edward Stanley was appointed Bishop of Norwich. Sedgwick at first felt dissatisfied, but soon came to befriend him.<sup>76</sup> Through this he was also friendly with so called 'Broad Churchman' A.P. Stanley. But we may note that Stanley's Life and Letters place it in this light:

'It was at Norwich that (Stanley) became attached to Canon Wodehouse, and to his colleague Canon Sedgwick, of both of

whom he always spoke with the warmest affection. It was at Norwich also that he laid the foundation of a friendship with Joseph John Gurney...<sup>77</sup>

Stanley was a complex person who had links with a number of quite different schools of thought, but his friendship with various Evangelicals need imply no identity of viewpoint.

One point which is of great interest is that the known reactions of some of Sedgwick's Evangelical friends to his religion was that they saw him as a man of essentially simple faith. Thus Carus, the successor of Simeon and a family friend of Sedgwick, said that his writings reveal:

'the religious side of his eminently Christian character, and show how a lively, steadfast and simple Christian faith was combined with intellectual gifts of the highest order.'<sup>78</sup>

On his death, the then Bishop of Norwich, the noted Evangelical Pelham, wrote to Canon Heaviside:

"His gifted intellect and strong common-sense, childlike simplicity of faith and loving warmth of heart, made him, to all homes and to all hearts where he was known to be much beloved.'<sup>79</sup>

His contemporary Evangelical friends, no less than his biographers, would have surely been astonished to hear a man with such a simplicity of faith called a 'Broad Church Radical'.<sup>80</sup> Simplicity of faith stands in the traditions of Thomas Wilson or William Wilberforce, but it is the very antipathy of the sophisticated (and often unintelligible) approach of Samuel Taylor Coleridge.

A final point about Sedgwick's particular Christian friends and associates concerns those with whom he chose to be linked in Christian projects. One particular project with which Sedgwick

was associated was the construction of Cowgill Chapel, completed in 1838. There were five trustees including Adam Sedgwick, three local people and the Reverend William Carus Wilson, who also made the address at its foundation.<sup>81</sup> Far from any kind of liberal churchman, Wilson was the son of the first gowmsman converted under Simeon, and was a Simeonite whose rigidity had been criticised by Simeon himself.<sup>82</sup> His blatant Calvinism had actually induced the Bishop of Chester to refuse to ordain him<sup>83</sup> and his dour and somewhat uncharitable brand of Calvinistic Evangelicalism was mercilessly exposed by Charlotte Bronte in the character of Mr Brocklehurst in Jane Eyre. A strange fellow traveller for a 'Broad Church radical'.

But the greatest Christian project with which Sedgwick chose to identify himself was the work of the missionary David Livingstone. After a religious conversion at the age of seventeen, Livingstone felt a call to missionary work, and in 1841 went to Africa as a missionary of the evangelically founded interdenominational London Missionary Society. Inspired by ideals similar to those of his eventual father-in-law Robert Moffat, and the noted Evangelical, Fowell Buxton,<sup>84</sup> Livingstone saw the evangelisation of Africa as being linked to its being 'opened up' and 'civilised', and it seems to have been the conviction that God's plan for him in the enterprise was more in the latter activities (rather than any changes in religious viewpoint) which led him to decide during his first furlough (1857-8) to become independent of the L.M.S. He did not wish to leave the L.M.S. open to criticism that Livingstone was not doing 'proper' missionary work.<sup>85</sup> It was during this furlough that Livingstone delivered his Cambridge lectures, later published with a prefatory letter by Sedgwick. Livingstone was, by this time, a celebrated national figure. He

was praised by Owen for his observations of fauna, Murchison for his contributions to Geography, and the Astronomer Royal for his celestial observations. Yet at Cambridge he upbraided his audience for its lack of support for the (mainly Evangelical) C.M.S. 'one of the noblest of our missionary societies'. Whilst he might be lionised by (say) Murchison as an explorer-scientist (and Murchison liked to think of his own worldwide geological exploration similarly!) we should note that Sedgwick focussed rather on Livingstone's completeness in recognising and serving God as Creator and Redeemer. In a farewell letter to Livingstone Sedgwick promises his prayers that God may 'support him in all his coming trials.' He continues:

'There is ~~but~~ one God, the God who created all worlds and the natural laws by which they are governed: and the God of revealed truth, who tells us of our destinies in an eternal world to come. All truth of whatever kind has therefore its creator in the will and essence of that great God who created all things moral and natural... all art and science, and all material discoveries (each held in its proper place and subordination), may be used to minister to the diffusion of Christian truth among men...'

Sedgwick rejoices in the great prospect of Livingstone's work in civilising Africa, bringing material benefit and abolishing slavery - but the 'greatest and true end of all' is to see the African's face turning heavenwards and 'after all his sufferings and our sins' calling him a Christian Brother. He concludes:

'May our Lord and Saviour bless your labours, and may his Holy Spirit be with you to the end of your life upon this troubled world.' 86

It is very much with the Christian missionary work associated with Livingstone that Sedgwick identifies himself.

### 2.3.5 Sedgwick on Church Parties

Sedgwick did not attach any 'party' label to himself and seldom talked in terms of church parties. He does, however, make some relevant comment, and in particular expresses views on some of the recognised 'leaders' such as Simeon, Coleridge and Newman. We noted in the previous part 2.3.4 the Wodehouse affair. As we look at Sedgwick's comments to him, we should bear in mind that Wodehouse believed that (because of human nature) it would always be possible to identify in the Church at any time three great categories of people: 'High Church, Liberal and Evangelical... under different historical names... Their principles have by turns predominated in the English Church...' <sup>87</sup> Sedgwick uses the corresponding terms (in 1840 and 1847) 'high-church', 'low-church' and 'evangelical'.

Writing in 1840 urging Wodehouse to stay in the Church of England, Sedgwick said:

'The correspondence I have had with you has disturbed me more than I know how to tell you. Alas! I can only repeat what I have written before. I wrote on the subject to Dr Arnold, and thought he perhaps might have been corresponding with you, but he tells me he is not. I think he will join you in your application. A very able and honest friend of mine, a kind and useful evangelical clergyman, talked over the points with me on Tuesday last. He wished the portions of the liturgy changed; but he added that he could not leave a church with which he agreed more nearly to the very letter, than with any other Church...' <sup>88</sup>

Sedgwick went on to add that this clergyman 'could not adopt the views of any extreme party high or low, &c &c'. Many of the High Churchmen would have wished to see Evangelicals like Gorham excluded, whilst Low Churchmen were reacting to the aftermath of Tract 90 in reciprocated hostility to Puseyites. Many moderate Evangelicals disagreed with the Tractarians but did not want a witch hunt.

Sedgwick's 1847 letter to Wodehouse contains an important passage worth quoting in full:

'I advise you to take the Archdeaconry by all means. There are no duties of the office you may not do with a good conscience; and you will do them if God spare your life with a good conscience. The office may enable you to do the cause of honest Scriptural truth much good - and your peculiar views about certain unfortunate passages in our Liturgy (in which I agree with you entirely) will not, and ought not, to interfere with your proper and energetic Archdiaconal duties. Let not good Evangelical men flinch, and refuse office. Now is the time for them to take the front rank, that the ultra high-church have gone over to the enemy. Julius Hare has some views like your own. He is a low-churchman according to the vulgar abuse of words. I should call him and you very high churchmen - men who are not content to bathe in tainted streams, but wish to steep themselves, soul and body, in the pure waters of life gushing from the fountain head. Ordinances of men are, or may be, good things in their way; and they are necessary for diffusion of the waters of life, and for irrigation - all this is plain to common sense.

Hare had some scruples, and stated them to his Bishop (Otter). The Bishop replied 'I can allow these objections to be of no weight, for my opinions on these points are just the same as your own. Therefore I again offer you the office, and I hope you will take it.' Now this is your exact position, if I mistake not. Act like Julius Hare, and take the office.'<sup>89</sup>

Sedgwick comes fairly near, in this passage, to identifying himself with the Evangelicals. Certainly he wishes 'good evangelical men' to take high office, though this is not necessarily to say he agreed with all their views. He is more explicit in a further letter to Wodehouse two months later:

'... I have finished Carus' Life of Simeon. It is a very remarkable book, and likely to do much good. 'Tis the history of a devout and faithful man, who stuck to his principles through evil report and good report, and ended by gaining the love and goodwill of all men about him. Pray read it soon. There are in it several letters on cases of conscience not very different from your own. Most of good old Simeon's views are wise and sound... He had many small faults, but he knew them and was humble under them, and after all they were motes in the beam of light, which only serve to show the track of the light more plainly to the senses. What a grand Christian death! And what a fine, eloquent, and Christian summary by the Bishop of Calcutta!'<sup>90</sup>

Many might, by 1847 and writing fourteen years after Simeon's death, profess some admiration for the man. But Sedgwick goes



further than this and speaks of agreement with most of Simeon's views. Few Arminian Evangelicals could have said more! The Wodehouse correspondence amply illustrates the basic theological position which has been argued for Sedgwick in this thesis.

Carus own later 'Reminiscences of Professor Sedgwick' in the Churchman of February 1889 quotes an even more significant passage in a letter from Sedgwick speaking of the Simeon biography:

"In reading your book I have constantly been struck by Simeon's good sense; and there is hardly a sentence on some doubtful questions of Christian counsel in the numerous extracts in the latter part of the volume, to which I do not give the assent of my heart. Some remarks on human corruption seem to me very true and good. I was blamed by some religious persons for one or two sentences in my sermon ... and I hope to consult you about an explanatory note (E) I have lately added. It seems to me that my statement, though it may be in less guarded language, is exactly what Simeon states in one of his letters (p.791). I once heard a statement of Robert Hall's quoted in conversation - I think it was at the rooms of my friend Musgrave (now Bishop of Hereford). Someone was using very strong expressions respecting the corruption of human nature. "True," said Hall; "take care that you go not beyond Scripture language and Scripture meaning. Totality admits not of degrees, and surely among men in a state of nature one may be worse than another." Our faculties of mind and body are in themselves good, for they are God's work. What we want is a new governing principle to guide them. If left to themselves they are implements of mischief; like an explosive engine without a safety-valve, and

without a hand to guide its movements. And how do we gain our governing principle ? From God Himself, not from ourselves. If we have faith we shall have sanctification, and holy purposes and powers. If we have them not, our faith is hollow - a babbling and a mockery of our Father. If we take a view in antagonism to what I have just stated, we are sure, if we be sincere men, to end in asceticism, or some form of monastic superstition... This was the great folly of the early centuries after Christ. The good old man, whose life is written in your book, said no such thing. He told us -(I cannot turn to the passage)- not to desert our post in the world but so to conduct ourselves in the world as shining lights; as men who, helped by God's Spirit, can turn their faculties to their Maker's glory.<sup>91</sup>

Simeon was sometimes described as a 'Calvinist', but himself disclaimed membership of any 'school' and was not one in the usual sense of the term.<sup>92</sup> Sedgwick here espouses similar views to Simeon, also citing Hall, professing not merely admiration (which is one thing), but positive agreement and alignment. Had Sedgwick really been some kind of 'Broad Church Radical' or 'Liberal Anglican' it would have been strange indeed to profess a heartfelt agreement with virtually all the pronouncements given on controversial religious questions by the acknowledged leader amongst Evangelical Anglicans of Sedgwick's generation. It must surely indicate that Sedgwick's sympathies lay elsewhere than current literature would suggest.

Sedgwick read widely and his writings quote a number of authors. On theological issues, however, he does not seem to quote 'Liberal' scholars, but old-fashioned High Churchman Marsh (on Tractarian issues),<sup>93</sup> and Evangelicals Hall and Chalmers.<sup>94</sup>

The Wodehouse correspondence also referred to several members of the supposed 'Broad Church', and to this we may now turn. Sedgwick indicates that he has been in correspondence with Arnold over the issue of subscription. Arnold was called by William Marsh 'an evangelical without knowing it',<sup>95</sup> and some of his views (though not all) were similar to those of Evangelicals. In this instance Sedgwick is referring simply to his views on subscription - which were generally known to be liberal. Of Hare, who was a friend of Sedgwick's, more mention is made. Sedgwick's characterisation of his friend should be looked at with care. Hare is a 'low-churchman', and possibly Sedgwick may be using this term as was then common in distinction to Evangelical. But he is clearly aware of its inadequacy, for he testifies to the sincere spirituality of both Wodehouse and Hare - and in the case of Hare his judgement would have been heartily endorsed by the Evangelical Rigg. As we have seen, Sedgwick is entirely correct in saying that Hare had some views very like those of Evangelicals like Wodehouse. Incidentally, the Bishop concerned (Otter) was known to have at least some sympathy for Evangelical views - and was a friend and biographer of E.D.Clarke, the man who stood with Farish on the platform at the first Bible Society meeting. But Sedgwick does not here claim to follow the particular views of Hare.

Around the time he wrote to Wodehouse (1847) Sedgwick also wrote a long letter to Hare, which is printed in Clark and Hughes. Again we need to notice exactly what Sedgwick says he agrees with. He refers to Hare's 'Note W', a vindication of Luther, and says: 'I thank you for this admirable note'.<sup>96</sup> He professes admiration for Luther, but adds: 'He was a man and not

an angel. I wish he had been less coarse. In more than half his disputes with Zuingle he was either wrong-headed or wrong...'

Again, this is the kind of comment which might have been made by many an 'orthodox' Protestant Evangelical.

References to Coleridge in the letters or works of Sedgwick are very rare indeed. One of the few comes in a context of criticism of the schools of empiricism of Locke and Hume for their apparent neglect of the innate faculties of the human mind. Sedgwick writes:

'The very knowledge we first acquire by experience, forces us by a true logical necessity, towards a contemplation of certain indwelling faculties of the mind, which must exist before all experience; and without which (as is well observed by Kant and Coleridge, and the whole modern school of Idealists,) experience itself would be impossible.'<sup>97</sup>

But this is a very passing allusion (followed shortly by comments highly critical of Kant). The philosophers actually cited by Sedgwick in opposition to the Lockians are Reid and Cousin - not Coleridge or Hare.

The only other reference of any significance (and indeed almost the only one whatsoever) comes as Sedgwick virulently attacks the 'slight damp suffusion of pantheistic vapour' from German philosophy which has temporarily obscured the 'pure philosophical diction' of England. Sedgwick says in a note:

'Few men have had a power over the English tongue more magical than that of Coleridge: but in the latter years of his life his philosophical essays sometimes become obscure and disagreeable, from his use of words, or turns of thought,

he borrowed from the German philosophy: and there are some

modern writers of great power, who seem as if they could never find it in their hearts to express themselves as Englishmen used to do. If the Germans are before us in any parts of philosophy, we may be compelled to adopt some of their technical language; but we have no need to smother our own language under it...'<sup>98</sup>

Since this is virtually the only reference made anywhere to Coleridge in extant works of Sedgwick, it is impossible to accept the kind of assertions we have seen (on p.5) about 'ardent admiration' for Coleridge, or a great influence of his thought on Sedgwick. Sedgwick reacted like most Evangelicals - with suspicion that Coleridge's language smacked of pantheism.

To Maurice, again, Sedgwick seldom refers. He states:

'I have once or twice seen Mr Maurice, and I know some of his dearest friends. I have read several of his books. His volume on The Prophets and Kings, and his volume on Sacrifice, were his latest works which I have read. All his works have one great charm - they have the savour of an honest, sincere, and truth loving mind. They all contain original thoughts, and original matter, which is another charm. But I cannot always go along with them without halting and stumbling. Sometimes I differ from him on points I think I understand; but, more frequently, I have a positive difficulty in understanding what he means. His thoughts run in a train so different from mine. I think also that he is often at fault on matters of practical wisdom; and (independently of Bible interpretation where the words seem to be against him) why did he disturb the congregation by his doubts and surmises on the very point which led to his removal from the Professorship at King's College? There was a want of practical

wisdom in this. And Sir James Stephen (our Professor of Modern History) committed the same blunder the year before. If Mr Maurice could have proved that he was right and that others were wrong - good and well! Let truth be told. But I defy him to prove, out of the Bible, that the punishments of God in a future state are limited in time. I dislike the discussion altogether, and I owe no thanks to Mr Maurice for disturbing our faith by making Hell into an universal Purgatory....<sup>99</sup>

Surely Brooke is not being fair to the context in his comment on this:

'It had also been gentlemanlike not to disturb the faith of the orthodox ... Sedgwick, for example, had a keen sense of 'practical wisdom', which means that he was unhappy on the way in which F.D.Maurice and Sir James Stephen had spoken out on the duration of Hell. It was not in good taste to 'disturb the congregation.'<sup>100</sup>

This makes it sound, perhaps unintentionally, as though Sedgwick is really unorthodox but thinks it ill mannered to speak out.

But this is surely not the case. Sedgwick would be happy for the 'truth to be told' - but thinks it unwise and unpastoral to disturb the congregation with speculation which cannot be substantiated.

The Bible is Sedgwick's authority here, and he believes it against the doctrine. It is also ironical to note that James Stephen was a leading Evangelical - nephew and admirer of Wilberforce and a statesman of renown. In the epilogue to his Essays in Ecclesiastical Biography (1849) he had expressed unwillingness to believe in everlasting torment, and claimed that 'Every argument, every narrative, every expostulation, every warning in the Bible would be as complete and as intelligible, if not emphatical without

it.' Unlike other main truths of Scripture its disappearance would leave the whole structure intact. A note in his diary amongst the Stephen papers for 8th September 1853 has him going round to Maurice's house 'to give him some advice about the scrape into which he has got.'<sup>101</sup> James Stephen was unable to help, and Maurice was dismissed a month later from his chair at King's College. Though Stephen presumably had little sympathy for the rest of Maurice's system, on this point at least it is interesting that Sedgwick is more 'orthodox' than an Evangelical heir of Clapham!<sup>102</sup>

On a more general level, Sedgwick's comments on Maurice are exactly what one might expect from an Evangelical viewpoint. He commends Maurice's sincerity (who can doubt it?), but often finds he disagrees with him. More important, he recognises that Maurice's thoughts 'run in a different train' from his own. The Coleridgean way of thinking of Maurice is quite foreign to the basically Evangelical way of thinking of Sedgwick. This applies to the atonement, and may be reinforced later with attitudes to Payleyan Evidences and the Bible, and natural theology.

Sedgwick, then, can be seen to have little sympathy with what is typically 'Coleridgean'. He dislikes Coleridge's Germanic philosophical influences (regarded by most people as Coleridge's main contribution), and finds Maurice on what one might call a 'different wavelength' from his own. His affinity for Hare may be explained by Hare's own position being closer to the Evangelical one on many issues.

To return to the issues of the Wodehouse correspondence, we may now look at Sedgwick's attitudes to the High Church and to Dissenters. The two issues overlap considerably. In a letter

to Ainger in 1850 he says that Ainger is 'too much of a high churchman for me', and defends the right of 'private judgement'.<sup>103</sup> Private judgement may be misused, like many other basic liberties, but this is no reason to deny or suppress them. In any event, 'forced unity is not spiritual unity', and to Sedgwick the 'good sincere Presbyterian is as true a member of the Catholic Church as is a member of the Church of England.' To Sedgwick this is the 'orthodox definition' of Catholic in the Church of England, but he adds:

'Don't think that I undervalue our church polity, and don't think it better than the Presbyterian. Tis not so. But Church polity is not Christianity; it is only one of the helps to it.'

In his 1868 Memorial about the Cowgill Chapel, Sedgwick outlines the sermon he gave at its founding in 1837. He says that the Church of England:

'...believed that their form of government and subordination of Church authorities was in nearest conformity with that which was sanctioned by the Apostolical teachers, and adopted by the earliest Churches of Christendom... there could be no doubt, I said, about our profession of doctrine. It was pure and evangelical. For every Minister of the Church of England had, before admission to his office, to declare, in public solemnity, his belief that the Holy Scriptures contain "all doctrine required of necessity for eternal salvation."<sup>1</sup>

Most of these ideas are repeated in the Preface to the 5th Edition of his Discourse. The great principles of the Reformers were the 'supreme authority of Scripture' and 'rights of private conscience.' The church liturgy and articles have no validity in themselves but



what they derive from Scripture.<sup>105</sup> The church is 'only infallible as she has drawn her doctrines from the Word of God.<sup>106</sup> She believes that her teaching is true - that her forms are apostolical and her doctrine evangelical.'<sup>107</sup>

With this kind of approach it is no surprise to find Sedgwick not sympathetic to the High Church. Of the Oxford School he was particularly critical, and when Newman and a number of others went over to Rome in 1845 Sedgwick wrote:

'Shame on them that they did not do so long since! ... I pity their delusion, I despise their sophistry, and I hate their dishonesty. Personally I know them not. It is not of persons but of principles I am speaking.'<sup>108</sup>

Sedgwick attacked aspects of Tractarianism more formally in the Preface to the 5th Edition of the Discourse. He pleads not to be misunderstood:

'I condemn not all the Oxford Tracts: for many of them are straightforward, just, and true, and well fitted for the times in which we live.'<sup>109</sup>

What he particularly objects to is the infamous Tract 90, which brought storms of protest from Evangelicals and Low Churchmen, and led to the suspension of the Tracts. Sedgwick regarded its attempts to reconcile the 39 Articles to Roman Catholic doctrines as sophistry, but went further:

'It is not so much the subtlety and sophistry, as the hideous immorality of Tract 90, that I wish to lay bare before the Undergraduates of Cambridge.'<sup>110</sup>

Newman is attacked personally, though not by name, Sedgwick says:

'His severe and ascetic life - his great scholastic learning in the track he had chosen for himself - his ignorance of

natural science, and his utter contempt for it - his subtle and entangled logic... all these had a seductive charm to some minds....' 111

Sedgwick accuses Newman of long concealing his true views, and adds that now (i.e. 1849) Newman's ideas of development are virtually pantheistic. He adds that:

'We have no right to blame, in a spirit of vituperative bitterness, a brother who changes his religious creed: but we have a right to demand from him an observance of the vulgar rules of truth and honour.' 112

This is perhaps not altogether fair to Newman, but it shows Sedgwick's thinking. He had a plain dealing man's abhorrence of anything which savoured of sophistry or deception. Thus, whilst he saw it as pastorally undesirable to disturb the congregation with unsubstantiatable speculation in the manner of Maurice, he would never countenance deliberate deception in simulating views which a person did not hold.

He was generally suspicious of the High Church, and writing to Livingstone in 1865 makes reference to 'Ritualism' - the offspring of Tractarianism:

'I greatly dislike the tendency to formal superstitious observances in the present day. Of course I am alluding to the High Church party in England. The idolatrous element is rife amongst us. We want to lean upon our own works and merits, and count them up as though they gave us the right to draw upon our Redeemer's treasures...' 113

Sedgwick's Memorial attacks those who reject the name 'Protestant' (a feature of Tractarianism going back to Froude), and urges:

'Let us cling, my Christian friends and countrymen, to the grand teaching brought to the light of day by our Reformers

- that every doctrine which is binding on the conscience as a rule of faith is to be drawn from the Bible - the fountainhead of all religious truth. Let us cling, on the same ground, to our common Protestantism, to the doctrine of justification by faith in the atoning sacrifice of the Son of God - a sacrifice at once sufficient, now and for ever, and admitting of no renewal and no supplement by any form of offering consecrated by the hands of man. But let not ours be an acquiescent and merely speculative faith (a mockery and a snare); but a lively faith, which under God's Spiritual law leads through communion with Himself (upheld by the humble use of the means of grace which He has Himself, through his revealed word, appointed for us) to sanctification of heart and a life of holiness and brotherly love.<sup>114</sup>

This, surely, is a basically Evangelical position ? There is no concept of priestly succession, or apostolic succession of any importance, and the Tractarians are attacked for tendency to a doctrine of works and for superstition. His attachment to the Church of England is because (like Simeon) he regards its articles and ordinances as truest to Scripture; but his emphasis is on the Bible and on justification by faith in Christ's atoning sacrifice. He emphasizes, however, that faith is not mere credal assent but personal response to, and relationship with, God. This, as we shall see, was also the basis of his relationship with Dissenters.

#### 2.3.6 Sedgwick and Dissenters

Sedgwick's feeling of fellowship with the Dissenters was not based on some vague tenets of natural theology or feelings of the creatorship of God. Dissenters of various kinds came together

to celebrate and even contribute to the building of the Anglican Cowgill Chapel with which Sedgwick was much involved. He said of them:

'... that they were united with us in a common Protestantism and a common protest of separation from the idolatrous forms and the priestly domination of the Church of Rome; that they were united with us in the great fundamental doctrines of Christian truth; that we had the same Bible, the same God, the same Saviour, the same ground of faith, the same Comforter to guide and help us through the darkest turns of our present life.'

He expanded on the common debt to the Protestant Reformers, and then:

'If in outward forms we continue divided, let our common Protestantism be our bond of Christian union in heart and love. Outward forms (excepting the two sacraments commanded in Holy Scripture) are but the fences and the scaffolding of God's Temple, and may be removed if the general good require it.'<sup>115</sup>

This feeling, however, did not extend to the Unitarians. On pp. 60-61 above we looked at Sedgwick's relationships with the Dissenters at Norwich - but also his strong reaction against Unitarianism. His feeling of fellowship with the (largely Evangelical) Dissenters was on a basis of common Protestant Biblical Christianity. The differences he saw largely as those of church polity and outward forms.

This underlies his attitude to Dissenters within the University. In the second quarter of the century Oxford did not admit Dissenters, whilst Cambridge would allow them to attend and study as long as they attended Church of England services, but would not allow them either to take a degree or hold a University post. In December 1833

Sedgwick's friend and fellow Evangelical, Professor Pryme, began a move to seek to allow Dissenters to proceed to a degree.<sup>116</sup>

Sedgwick soon became involved, both in letters and in petitions.

Writing to The Times on 8th April 1834, he laid out his case. Much of his argument centred around the fact that before the time of James I religious tests were not requisite in order to take a degree. Sedgwick argues that James introduced them by an 'informal' royal letter, in a high handed way, and 'grievously against the wishes of many of the then members of the Senate.'

In his letter, a second one of 18th April, and a letter to Bishop Blomfield of 27th April, Sedgwick clarifies what he wants. Firstly, no Dissenter would have an unqualified right to attend Cambridge. The requirement to attend Church of England services is known:

'A moderate, well-informed Dissenter will come up under such a system... and he will take a degree. A bigot - a man who would haggle about organs and surplices - will and must keep away, and we do not want him...'<sup>117</sup>

Commenting on this passage, Winstanley makes the puzzling statement: 'Adam Sedgwick, like many Broad Churchmen, was extremely intolerant of religious scruples which he did not share.'<sup>118</sup> Aside from the fact that according to many, 'toleration' is the mark of a Broad Churchman, the criticism of Sedgwick is misleading. Sedgwick saw the reality of Christianity in a life in relationship with God through Christ based on a Biblical faith. Ordinances, of whatever denomination (and though he himself thought the Anglican ones best) were only helps to this - and to object to attendance at Christian services because of music or clothes seemed to him absurd. On the other hand, though he himself subscribed to the 39 Articles, he did recognise that they contained doctrines beyond those necessary for a Biblical faith, and sympathised with Dissenters whose scruples

prevented them subscribing to articles of belief which they did not believe. His position in all this was both sensible and consistent.

Sedgwick also believes that Dissenters will have no objection to attending theology lectures. Characteristically, however, in controversy he overstates his case. Attendance is not compulsory, and (he claims) in any case no such lectures in the last 30 years have been such that 'a Dissenter of any denomination would have scrupled to attend.' He continues:

'... such lectures being studiously confined to a critical examination of various parts of the New Testament, to discussions on the evidences of Christianity, and so on... Paley's Evidences, Butler's Analogy, and Doddridge's Evidences, have been for many years the subjects of college divinity lectures, in addition to the Greek text of the New Testament. The subjects of controversial theology have been carefully avoided...'<sup>119</sup>

Sedgwick's statements were challenged, e.g. by William Selwyn with whom Sedgwick engaged in polemics via the press. On May 30th the Trinity Divinity lecturer, R.W. Evans, issued a statement asserting that his New Testament lectures went beyond the philological and dealt with doctrine. A copy of this exists in the University library with a handwritten note in Sedgwick's handwriting saying that the Divinity lecturer has failed to state that his lectures 'were not compulsory - and that they were attended by a very small class...' and so are irrelevant.

It is not clear at this time quite how far Sedgwick wished to allow Dissenters to hold posts in the University. To Blomfield he wrote:

'By the Law of the Land no person without signing himself a member of the church of England can be admitted a Fellow of a

College or a Professor of a University. The Petitioners did not contemplate a change in this law. 14. Were this law repealed, members of the University refusing to sign the tests of James I would be admissable to no fellowships except such as are now held by laymen. Of these there are comparatively few...<sup>120</sup>

How far Sedgwick personally wished Dissenters to be allowed lay posts is hard to say. When the issue resurfaced in 1869 it was (with Sedgwick's support) clearly designed. Clark and Hughes saw no change in Sedgwick's position; though Garland has questioned this.<sup>121</sup> any difference does not seem very significant. Sedgwick never seems to have really contemplated the possibility that the University could cease to be fundamentally a Church of England institution - let alone that it could become 'secularised'. At the same time, however, he recognised that should the intellectual life of the nation ever cease to be Anglican in nature, then Cambridge could preserve its Anglican nature only at a cost of becoming an intellectual backwater. This touches, also, upon his view of science as essential to the life of a properly intellectual establishment like Cambridge. He notes that three quarters of 'those who, as Professors, are employed in carrying on the scientific work of the University' are in favour of abolishing tests.<sup>122</sup> He fears that, if not granted, the Faculty of Medicine may be 'lopped off from us' - and bewails the catastrophic effect this will have on scientific cooperation.<sup>123</sup> He adds: 'the scientific character of Cambridge is not only its honour but its security. As a great learned and scientific University giving degrees in all the learned faculties... Cambridge may stand firmly... But if she once be considered as a mere school for the Church Establishment her endowments will be thought out of all reasonable dimensions...' If this happens, before long the edifices will crumble. In 1869 Sedgwick was reported to have said a similar thing: 'I am a

Churchman because I believe the Church of England to be right; but I deprecate the University hiding itself in any little nook of prejudice out of the general spirit of the community.'<sup>124</sup> If, then, Dissenters should command a predominance of the intellect of the nation, let them predominate.

In 1834, on April 18th, a Bill was introduced to abolish tests - it passed the Commons but not the Lords. In that year, however, Dissenter bitterness at Anglican privilege reached a peak.<sup>125</sup> Thus Sedgwick wrote to a friend on 28th May:

'The Dissenters have now, if not unanimously, yet without... contradiction from any of their body, avowed that their great and ultimate object is the overthrow of the church... I do not see that you or I, or any member of the Church of England, can or ought to be called upon to do an act which must manifestly tend to our own destruction... I am sorry that the Dissenters did not meet the Churchmen in a better spirit... Now, I am afraid, the coals of enmity are blown into a blaze by the ordeal on both sides - and we must wait till they have burnt out...'<sup>126</sup>

Though, however, Sedgwick might now feel it prudent to 'lie low' on the issue, his words were still causing alarm in some circles.<sup>127</sup>

The British Critic, in July 1834, chose to review Sedgwick's Discourse amongst a set of papers dealing with the Dissenter issue.<sup>128</sup> It does so because it sees his attitude to admission as in conflict with the idea of a university presented in the Discourse. Its opinion of Sedgwick is high: 'his name alone gave to the Cambridge Petition the pernicious power it possessed.' Sedgwick has, it says, a high reputation in the scientific world, an 'ornament as well of the University as of his college.' Sedgwick, 'although greatly zealous in the pursuit of scientific truth' values far higher than all other religious truth, and would sacrifice his very existence rather than



be a means of his listeners wavering in their attachment to 'the pure faith of that Church whose doctrines he was then inculcating.' The British Critic, however, doubts that Fellowships and College Offices will long be restricted to Anglicans once Dissenters are allowed Cambridge degrees. Then, it fears, there is strong likelihood of Arian and/or Unitarian Tutors 'spreading around them a baleful influence', though Sedgwick may not want or envisage this. The reviewer cites Sedgwick's own eloquent words about worshipping at the same altar where past greats worshipped. Only in a system of uniform worship and religious teaching is this kind of education possible. Thus Sedgwick's wish to admit Dissenters contradicts the vision in his Discourse.

Strictly, of course, there is no contradiction. Sedgwick in fact believed that both uniform Anglican worship and uniform Anglican teaching would continue if Dissenters were admitted to degrees. The British Critic clearly did not feel Sedgwick was realistic. The same attitude, in fact, was heightened in the later British Critic review in 1839 of the BAAS, after the journal had come under the control of Newman and his circle. There was no quarrel with science as such, but as it happened the same group (of which Sedgwick was a leading figure) were involved in the BAAS and awarding honorary degrees to Dissenters, and agitating for the admission of Dissenters to degrees. Dissenters entertained in Oxford and Cambridge would form: 'a desire to participate in permanent ownership of those institutions, - albeit this would only be brought about by secularising them, and severing from Christianity the general conduct of education within their walls.'<sup>129</sup> Newman, wrestling later with the same problem, comes to the conclusion of the reviewer that universities should be denominational. Ultimately, of course, they were right in that secularisation was the result of the process begun when Sedgwick's wishes were fulfilled in 1871. But had

Newman and his circle in 1839 ultimately triumphed perhaps Sedgwick would have been right and Cambridge ceased to be a leading institution of education. Ultimately, the controversy illustrates Sedgwick's optimistic view that truth (the Anglican version of Christianity) would hold its own in the intellectual world without artificial protection.

2.4 Notes

1. Clark & Hughes, 1, p. 38.
2. In the Cumbria County Archives, Kendal.
3. Sedgwick, Memorial p. 60-1; Clark & Hughes 1, p. 39-40.
4. Sedgwick, Memorial p. 67; Clark & Hughes, 1, p. 41.
5. W. Carus, Memoirs of the Life of the Rev. Charles Simeon, p. 692.
6. Memorial, p. 68.
7. Memorial, Supplement p. 43.
8. To Lady Affleck, 18.2.1859, Trinity Whewell Collection Add Ms a 213.
9. Clark & Hughes, 1, p. 39.
10. See e.g. J. Pollock, Wilberforce, ch. 5 and p. 101 for the 1790 election in which Wilberforce was returned without poll; also R. Furneaux, William Wilberforce, who dates his 'conversion' to 1785 (p. 54) and involvement against slavery to 1787 (p. 60). The Clark & Hughes note to the 1784 election, taken from the Life of Wilberforce by his sons is evidently mistaken.
11. Wesley wrote Thoughts Upon Slavery in 1774 and wrote to both Wilberforce and Sharp.
12. C. Smyth, Simeon and Church Order, p. 25.
13. Ibid., p. 25-26.
14. Ibid., p. 31-32.
15. Clark & Hughes 1, p. 41.
16. Smyth (ref. 12), p. 25; Sedgwick's practice is recorded in Clark & Hughes 2, p. 586; Whewell, in contrast, followed a High Church pattern of a formal service and litany (Smyth, ref 12, p. 32n).
17. See e.g. The Oxford Dictionary of the Christian Church, Encyclopædia

- Britannica (11th Edn), Chambers Encyclopedia, C.J. Abbey and J.H. Overton, The English Church in the Eighteenth Century, J. Keble, The Life of the Right Reverend Father in God, Thomas Wilson D.D., C. Cruttwell, The Works of the Right Reverend Father in God Thomas Wilson D.D., H. Stowell, The Life of Thomas Wilson.
18. Russell, A Short History of the Evangelical Movement, pp. 2-3.
  19. Abbey (ref. 17), 1, p. 142.
  20. J. Keble, The Life of ... Thomas Wilson D.D., (Oxford 1863).
  21. e.g. Sermons p. 24: 'Salvation is indeed offered to all to whom the Gospel is or has been preached; but not all are disposed to receive it, especially on the terms on which it is offered.'  
p. 127: 'The greatest sinner may be sure of pardon if he repents and brings forth fruit answerable to amendment of life.' Stowell also shows Wilson's concern for plain words and for the poor.
  22. Sermons p. 133.
  23. Sermons p. 140.
  24. See note 21, also Sermons p. 186.
  25. e.g. Sermon xx; he translated parts of it into Manx.
  26. Carus (ref. 5), p. 8-9; H.E. Hopkins, Charles Simeon, p. 28; M. Loane, Cambridge and The Evangelical Succession, p. 177, etc.
  27. Moule, Charles Simeon, p. 17; Wilson, A Short Account... of the Lord's Supper, p. 19. Moule omits the last  $4\frac{1}{2}$  lines containing the strongest part of Wilson's statement.
  28. Clark & Hughes, 1, p. 43.
  29. See Carus: (The Churchman, 1882, 3, p. 225-237), where both Sedgwick's brother and his sister testify to this (pp. 229-230 and 235). For this as typical of Evangelicals, see F.W. Cornish, A History of the English Church in the Nineteenth Century, p. 19,

30. Memorial Supplement p. 40.
31. Memorial Supplement pp. 55-58; Clark & Hughes 1, p. 57 & 232.
32. Clark and Hughes 1, p. 68.
33. Lonsdale Magazine 1821 vol. 2 no. 13 p. 19; see also the DNB and Speakman, Adam Sedgwick, ch.4.
34. Memorial, p. 78.
35. Memorial, p. xiv.
36. Clark & Hughes 2, p. 589.
37. Amongst the Sedgwick papers in Cambridge University Library.
38. H. Gunning, Reminiscences of the University Town and County of Cambridge, 1, p. 246.
39. J. Hartford, Recollections of William Wilberforce, p. 67; Pollock (ref. 10), p. 240; Furneaux (ref. 10), p. 354.
40. Clark & Hughes 1, p. 496.
41. Clark & Hughes 1, p. 496.
42. Unpublished Letter: Cumbria County Archives, Kendal.
43. Clark & Hughes 2, p. 442.
44. Clark & Hughes 2 p. 325; the letters in the County Archives show by their wording that there were none others between the three extant.
45. e.g. Dictionary of National Biography, also Carus' article (ref. 29), on both his piety and his prayer life. (See also below.)
46. Discourse 1st Edn., p. 50.
47. Discourse 5th Edn., p. 58 carries the identical passage to the 1st Edition, but the appendix p. 137-138 elucidates Sedgwick's basic argument which is that if man were really so totally

depraved that he had no conception of a Godhead or of a 'moral law to which he was bound to give obedience' then 'being without law, he would, in the language of the Bible, be without transgression.' But he strenuously denies that he has ever asserted: '(as I have been made to do by a pretended but false quotation from this Discourse) that man, as a religious being, can be converted from evil to good by the mere natural power of persuasion acting on the moral elements within his bosom, and without spiritual help.' (p. 138). All this is unexceptional Evangelical Arminian theology. The fanatical views of total depravity against which he has been contending are 'fanatical, mischievous and untrue - and they naturally end in uncharitable judgments, in sour ascetical religion, or in some form of monastic superstition.' Though he names not Calvinism, this is a classic response to it. In a similar connection on p. 169 of the Discourse he does name the arch-Calvinist American philosopher Jonathan Edwards (though accepting his piety). He contrasts the views of non-Anglican Evangelicals Hall and Chalmers, whose views on such matters he basically follows (p. 158, also p. 168).

48. Cole (Popular Geology Subversive of Divine Revelation) is actually quoted in the 5th Edition Discourse, though not named (p. 139); the Christian Observer was one important organ which (though disliking Cole's tone and his obscurantism) had been misled by him into expressing some doubts about Sedgwick's complete orthodoxy.
49. Carus' article (ref. 29), reproduced this passage from the Discourse and a letter in which Sedgwick said 'I hope to consult you about an explanatory note (E) I have lately added' - claiming that his own views were identical to Simeon's. See also below.

50. Discourse, 5th Edition, p. 135.
51. Memorial p. xviii-xix, quoted below on p. 78.
52. Discourse 1st Edition p. 78, repeated all Editions.
53. Discourse 1st Edition p. 80, repeated all Editions.
54. Discourse 1st Edition p. 81, repeated all Editions.
55. Cannon, Science in Culture, p. 53.
56. We have noted Newman on Arnold, and also Rigg's admiration of Hare and even to some degree Maurice.
57. Bunsen was a friend of Arnold and Hare, but saw himself as having natural links with the English Evangelical Alliance (formed in the mid 40's). He also worked closely with Shaftesbury (1841) over the appointment of a Jerusalem Bishop, and with Henry Venn the younger as secretary of the CMS on the African Languages (see Hennell, Sons of the Prophets, pp. 45-6, 85). The English Evangelicals were suspicious of some of his views, but admired him as a man and a Christian.  
  
On the importance of family links, it is interesting to note that Shaftesbury was a cousin of Pusey and a brother in law of Cowper (from whom, in the Palmerston era, he felt 'poles apart'; see Hennell p. 57-8).
58. Cannon (ref. 55), p. 51.
59. Letter Ms 213/47 in the Whewell Collection, Trinity College.  
  
Similarly in Clark and Hughes, 2, p. 405: 'Dear Romilly was the oldest friend I had in Cambridge. Indeed he was the only one left of those with whom I was on close terms of brotherly love during the early years of my academic life. A cross look or a cross word never, I believe, passed between us; and our intimacy became closer and closer as we advanced in life. He was a Christian indeed, without selfishness, without guile ... To him

I could ever unburden my heart, more than to any other Cambridge friend, in my hours of doubt, or anxiety, or sorrow....' Clark and Hughes, 1, p. 281., call Romilly 'undoubtedly Sedgwick's dearest friend' amongst Trinity Fellows.

60. Clark & Hughes, 2, p. 196.

61. Ibid., p. 269.

62. A. Bayne, Autobiographical Recollections of George Pryme Esq., M.A., p. 354.

63. Ibid., p. 26.

64. Ibid., p. 141. H.E. Hopkins in Charles Simeon, p.201-2 says that at this time there was no standard Evangelical line on such matters - Sedgwick himself speaks of a later hardening of attitudes.

65. Carus (ref. 29), p. 275.

66. Clark & Hughes, 2, p. 392. It was through Mrs. Clarkson that Sedgwick first learned of the private lives of the various Lake poets including Wordsworth, Southey and Coleridge (Clark & Hughes, 2, p. 427).

67. Thackray and Morrell, Gentlemen of Science, p. 28.

68. Cornish, A History of the English Church in the Nineteenth Century, pp. 52 and 404. Bathurst, like Sedgwick and his fellow Evangelical, Pryme, was also a Whig in politics.

69. Clark & Hughes, 1, p. 437.

70. F.K. Brown, Fathers of the Victorians, p. 493, points out that Gurney is one of the handful of friends mentioned frequently by Wilberforce who was outside the circle of Evangelical Anglicans.

See Hopkins, Charles Simeon, p. 165-7; Moule, Charles Simeon,



p. 209 quotes a letter from Gurney to Simeon which finishes:

'I am thy faithfully attached son in the Gospel - for I will esteem thee as a father.'

71. J.B. Braithwaite, Memoirs of J.J. Gurney.
72. See Thackray and Morrell (ref. 67), p. 28.
73. Clark & Hughes, 2, p. 449.
74. Clark & Hughes, 2, p. 20.
75. Wodehouse, An Address to the Clergy of the Archdeaconry of Norfolk (1847).
76. Clark & Hughes, 1, p. 485.
77. R.E. Prothero, The Life and Correspondence of Arthur Penryn Stanley, 1, p. 252.
78. Carus (ref. 29), p. 225.
79. Ibid., p. 234; Shaftesbury described Pelham as 'Decidedly of the Evangelical School' (E.H.Hodder, The Life & Work of the Seventh Earl of Shaftesbury, 3, p. 197); he came to Norwich in 1857, remaining until well after Sedgwick's death. Hennell (ref. 57), describes his reforms at Norwich.
80. Further testimony to Sedgwick's religious life comes from his fellow Canon C.K. Robinson, in Clark & Hughes ch. ix.. Winstanley, in Later Victorian Cambridge, p. 1, says of Robinson that he was 'much admired in evangelical circles for his piety'.
81. Clark & Hughes, 1, p. 487, Memorial p. 6; the other Trustees were Sedgwick's brother John (Incumbent of Dent), John Elam of Dee Side, and Edward Bannister of Cowgill.
82. F.K. Brown (ref. 70), p. 456, etc. gives much critical

comment on Wilson.

83. The story is told in Hopkins (ref. 26), p. 98, Allister (in Samuel, Evangelical Succession) p. 70, etc., see also Chadwick, The Victorian Church, 1, p. 446.
84. Fowell Buxton was a kind of Anglican Quaker. Hennell states: '... it was the ideas Buxton expounded in his book, and at the euphoric meeting of 1 June 1840, which were to find fulfilment in the next thirty years. In the audience of that meeting was David Livingstone, then a medical student at Charing Cross Hospital. There is a straight line from Buxton's speech in Exeter Hall in 1840 to Livingstone's speech in the Senate House in Cambridge in 1857.' (ref. 57), p. 27.
85. See W.G. Blaikie, The Personal Life of David Livingstone, chapter xl.
86. Partially cited in Clark & Hughes, 2, p. 339, printed in full in Blaikie (ref. 85), p. 239.
87. Wodehouse (ref. 75), p. 35-36.
88. Clark & Hughes, 2, p. 21. I have been unable to trace this Arnold correspondence. In a letter to Dr Butter, 9th May 1834 (British Library see under Pressmark ADD 34589) Sedgwick suggests that it may be helpful on the issue of Dissenter admission to degrees if Dr Arnold and a few 'good men concerned in Education and well known to the world' would support their position. Butter replies (May 11th) having heard from Arnold, agreeing that he is an honourable and respectable man, but stating that Arnold's views differ on the issue from Butter and Sedgwick. But the fact that Arnold and Sedgwick might be prepared to work together on common

causes does not imply any general similarity of theology.

89. Clark & Hughes, 2, p. 114.
90. Clark & Hughes, 2, p. 122
91. Carus (ref. 29), p. 231
92. Sedgwick's friend and fellow Evangelical, Pryme, who says that his views were basically similar to those of Joseph Milner who had taught him, says that Simeon 'positively disclaimed' to him the belief in particular predestination as understood by the Calvinists (see A. Bayne, Autobiographical Recollections of George Pryme Esq. M.A., p. 26 and p. 141). His disclaimer of belonging to any school is well known. The general drift of Simeon's circle away from Calvinism is reported by Hennell (ref. 57), pp. 8-9.
93. Discourse 5th Edition, p. cccxxi.
94. In the 5th Edition Discourse both Chalmers and Hall are recommended on p. 158, Hall again on p. 168, and various of Chalmers' sermons on p. cccxix, & p. 55 and p. 115. Sedgwick also (as we shall see) thought his position on theological aspects of science similar to the Evangelical Hugh Miller.
95. From The Life of the Rev William Marsh, p. 239. Marsh was a leading Simeonite and personal friend of Simeon.
96. Clark & Hughes, 2, p. 112.
97. 5th Edition Discourse.
98. 5th Edition Discourse cclxxxiv.
99. Clark & Hughes, 2, p. 295 (20th January 1855).
100. L.J. Jordanova and R.S. Porter, Images of the Earth, p. 52.
101. Hennell (ref. 57), p. 102.

102. On this particular issue it might be added that today also there are leading Evangelicals who disbelieve in eternal torment, sometimes without publicising the fact. They do not, of course, share Maurice's other elements of theology.
103. Clark & Hughes 2, p. 196.
104. Discourse 5th Edition, ccclxxi.
105. Ibid., ccclxxi.
106. Ibid., ccclxxvii.
107. Ibid., ccclxxviii.
108. Clark & Hughes, 2, p. 94.
109. Discourse 5th Edition, ccxcv, Sedgwick's reference is to their 'Tracts for the Times'.
110. Ibid., ccxciv.
111. Ibid., ccxcii.
112. Ibid., ccxcii.
113. Clark & Hughes, 2, p. 411.
114. Memorial, xix
115. Ibid.
116. Clark & Hughes explain in some detail the various steps involved, and see also Garland, Cambridge Before Darwin.
117. Clark & Hughes, 1, p. 421.
118. Winstanley, Early Victorian Cambridge, p. 94.
119. Sedgwick, letter to The Times, 8th April, 1834.
120. Sedgwick, letter to The Times, 18th April, 1834.
121. Clark & Hughes, 2, p. 451, Garland (ref. 116).

122. Letter to The Times, 8th April, 1834.
123. Ibid., also Clark & Hughes, 1, p. 421.
124. Clark & Hughes, 2, 451.
125. See Chadwick, The Victorian Church, 1, p. 61.
126. Letter to Dr Butter, May 28th 1834, in the British Library,  
Pressmark ADD 34589.
127. British Critic, 1834, 31, 186-199.
128. British Critic, 1839, 39, p. 42.
129. Newman, The Scope and Nature of University Education (1852).

### 3.1 Biblical Inspiration and Accuracy

#### 3.1.1 Introduction and 'Ideal Types'

To those who believed that the Bible was in some sense a revelation of God, there were two basic questions. The first was what kind of mechanism was involved in the 'inspiration' of the Scriptures. The second, which was related to this, concerned the kind of accuracy one would expect in the Bible on specific historical or scientific details.

On Biblical inspiration it will be useful to set out a number of what might be called 'ideal types' of view. Actual individuals may not fall exactly into these categories, or their position may be uncertain, but they are useful as a conceptual framework. In actual fact in this particular instance there usually are individuals, both historically and today, who fall fairly well into each category.

##### (a) The 'docetic view'

This is a view that God, as it were, dictated the Bible to those who wrote it down. Thus Hooker: 'They neither spake nor wrote any word of their own, but uttered syllable by syllable as the Spirit put it into their mouths.'<sup>1</sup> By implication the Bible, as originally given, is not only free from all kinds of error, but details of its language will reveal facts about history and science unknown to its human writers. The view has a long history, and 'Scriptural Geologists' have generally arisen from amongst those who hold such a view, from the beginnings of geology until the present day. There is obviously a need to explain away any historical or scientific discrepancies, and this is often

done by denouncing orthodox historians and scientists.<sup>2</sup> An alternative name for the view is 'plenary inspiration', or sometimes 'verbal inspiration' (though either may be ambiguous).

(b) The 'inerrancy' view

This holds that the message of the Scripture writers was totally inspired, but that they used their own language to express it. In this view, God, in his providence, would have prevented any actual scientific or historical error from appearing in the Bible as it was originally given and written down; in this respect it could be taken as being as free from error (in Simeon's words) 'no less than if their very words had been dictated from above.' But nevertheless it is couched in their own language, it is not literally by God's verbal dictation. Thus the writers used 'observer language', referring for example to 'sunrise' without implying a geocentric system. It is not meant to teach us science, and we may be suspicious of attempts to make it do so. There is still a need to explain any apparent contradictions with history or with science, but the Bible is not seen as a source book for science.<sup>3</sup>

(c) A 'selected area' view

This holds that the Bible is infallible on points of theology or morality but not on points of science and history. The Bible is still seen as, in some sense, a book through which God reveals himself in an objective sense, but obvious difficulties exist as to exactly how much historically is reliable. Judae-Christian religion is based on a view of God acting in history - both in his dealings with Israel and supremely in the incarnation, death and resurrection of Christ. It is a historically based faith. What then is to count as a detail and what as an essential? Within this view, then, there may be a whole continuum of

positions, from those who accept errors in the details of names and places, to those who see the whole Bible in a context of an outdated cosmology and accept that major historical narratives are myths.<sup>4</sup>

(d) A 'subjective' view

This is a view that the Bible contains the Word of God but is not to be identified with it. Scripture witnesses to the reality of revelation, and is in some sense a permanent possibility of it, but is not propositionally reliable. Rather, it is the primary means which God uses actively to speak to individuals who read it, but illuminating them individually. Obviously, this removes any necessity to try to reconcile scientific or historical discrepancies, but it leaves problems. God is seen as an active agent in illuminating the individual, but the Bible remains a human product. We are left with no methodology or source with which we might establish even a rudimentary reliable religious propositional system. Objective religious fact becomes impossible, or at best very difficult to defend. The key is rather what 'finds me' or 'becomes the Word of God to me' as I read the Scripture. There are often several paradoxes involved in the view. First, although the Bible itself may not be seen as merely propositional, most theologians are unable to move away from propositions into pure subjectivism - even the most extreme existentialists. Secondly, though there is a certain fuzziness about how far the Bible may be propositionally reliable, it is a highly venerated book - even to the extent where less is made of the use of reason in evidences and natural theology than (say) by Evangelicals.<sup>5</sup>

(e) A 'man-based' view

This holds that the Bible contains a record of some of man's



searchings for religious truth. But in writing these down there were included a number of legends, and also 'myths' in the sense of pre-scientific conceptualisations of primitive man of his religious experiences. Now that man has 'come of age' (in our present day of course) the Bible needs to be 'demythologised' in order to get at the kernel of religious experience and truth. Those who hold such a view often see miracles as contraventions of the laws of nature, and so assume they could not have happened in a literal sense. Rather, they are to be seen as 'valid' expressions in a pre-scientific culture of certain views about the nature of those to whom they are ascribed. To see Jesus for our own generation he needs to be 'demythologised', i.e. to be reprotayed in the (non-miraculous) way we would see him if he lived the same life today.<sup>6</sup>

### 3.1.2 Reformers and Evangelicals on Inspiration

It is useful to set out various views as 'ideal types', though it is by no means easy always to identify a person with a particular viewpoint. On the Reformers, for example, scholars disagree, and range from A.L.Lilley who said of Luther: 'No Christian Doctor of the front rank ever disparaged the revelational role of the Scripture more consistently than the great Reformer'<sup>7</sup> to Barry who accuses Luther and Calvin of 'bibliolatry'.<sup>8</sup> Luther did speak of the Bible as the 'Word of God' and believed it inspired. But, as Warfield commented: 'The Reformers, though using the language conformable to, or even suggestive of, the theory of dictation, do not formally present that theory, as do the Systematists of the seventeenth century...'<sup>9</sup> The Puritans, on the whole, tended to accept view (a). John Owen, for example, wrote: 'They invented not their words themselves, suited to the things they learned, but only expressed the words they received.'<sup>10</sup> Matthew Henry, commenting

on 2 Peter 1.19-20, wrote: 'The Holy Ghost inspires and dictates to them... so that the very words of Scripture are to be accounted the words of the Holy Ghost...'

What was the view of the Evangelicals in the period of our present interest ? Certainly there was no shortage of those who accepted view (a), 'plenary inspiration'. In particular the Recordites may be associated with such a view - as Conybeare himself noted.<sup>11</sup> Conybeare notes the extremism of the Recordites, but confuses and confounds what we have called view (a) and view (b) above. Perhaps this has encouraged statements like that of Thackray and Morrell that Evangelicals were 'given to a literal interpretation of Genesis.'<sup>12</sup> In any event, moderate Evangelical leaders were more circumspect.

Wesley held strongly to the inspiration of the whole of Scripture.<sup>13</sup> But he seems not actually to specify any details of the mechanism, and there is no proof that he held any theory of dictation.<sup>14</sup> Simeon is unguarded in the way he expresses the doctrine. Thus in one place he writes that the 'whole Scripture was as much written by the finger of God as the laws were, which he inscribed on the two tables of stone...'<sup>15</sup> More carefully he says that the Holy Spirit kept the apostles: 'free from error of every kind, so that which they have spoken must be regarded as the Word of God, no less than if their very words had been dictated from above, yea, in all that they revealed, they were kept from error of every kind and every degree.'<sup>16</sup> But elsewhere Simeon adds that though the Spirit preserved them from error, yet they 'still expressed themselves in their own way.'<sup>17</sup> Simeon, then, does not believe that God mechanically dictated the Scripture, but that in regard to being free from error they are as accurate as if he had.

Simeon would not have tolerated any historical inaccuracy, but it is far more open how far he expected scientific accuracy rather than a simple use of 'observer language'. His modern biographer argues that his view was open, and cites his words: 'no error in doctrine or other important matters is allowed- yet there are inexactnesses in reference to philosophic and scientific matters because of its popular style.'<sup>18</sup> Simeon was not a systematiser, and attempts to work out precisely a line between 'doctrine' and 'scientific matters' would not have appealed to him. He did, however, appreciate Chalmers and Whewell, and was open to the possibility of the 'gap theory' (which incidentally was Sedgwick's own viewpoint).<sup>19</sup>

If we look at non-Anglican Evangelicals, Chalmers actually commends the words of Owen.<sup>20</sup> In practice, however, it is very clear that Chalmers did not expect scientific language from the Bible. He fully allows for the general nature of Biblical language.

Another interesting Evangelical is the Dissenter John Pye Smith. At one time he actually doubted whether the Song of Songs and book of Esther should be included in the canon at all<sup>21</sup> though later he was convinced. On inspiration he says:

'the essential seat of inspiration lies in the thought and sentiments, whatever be the variety of phrase by which they be expressed... that though the inspired writers use the diction which is most familiar to them, according to their characteristic style and verbal habits, yet in all cases the original Hebrew, Chaldee or Greek is the best expression of the mind of the Spirit. For either the very words and phrases were suggested by the Holy Spirit (most probably so in prophetic declarations and messages) or were accepted, so to speak, by Him; which two cases are precisely the same in practical effect.'<sup>22</sup>

In a series of lectures (published 1838) Pye Smith illustrated the accounts of the creation and deluge with reference to modern geology. He recommends the works of Ray and Derham, as well as Whewell and Herschel and Buckland showing (as we shall again note) full acceptance that the words of Scripture were observer language, and needed interpreting in the light of scientific knowledge.<sup>23</sup>

### 3.1.3 The Broad Church and Coleridge on Inspiration

As we turn from Evangelicals to the so called 'Broad Church' we find an extremely heterogenous group of people. Copleston, the 'father of the noetics' was said by Simeon to be nearer his views of Scripture than anyone else he had ever met.<sup>24</sup> Even allowing that Simeon may not have understood Copleston fully, Simeon was not a simpleton and his remark must have some foundation. At the other extreme, Baden Powell espoused an opinion along the lines of view (c), which by the time of the Essays and Reviews had led him to see accounts of miracles as virtually an unacceptable embarrassment. The Coleridgeans also present certain problems. In particular there is disagreement amongst scholars as to whether Coleridge was basically following a view along lines of (c) selected area, or (d) subjectivist.

It may be best to begin by seeing how far the orthodox and Evangelicals could go along with Coleridge in his views of inspiration. J. Robert Barth helpfully compares Coleridge's view with that of the orthodox Anglican William Lee who wrote The Inspiration of Scripture (1857). Lee, like Coleridge, distinguished between the work of the Eternal Word in 'Revelation' and the work of the Holy Spirit in 'Inspiration'. Barth emphasizes the importance of this distinction in distinguishing what in Scripture is known only by supernatural revelation (for example Christ's incarnation) and what is known by natural knowledge (e.g. that there was a census taken in the

year of Christ's birth). Lee believes: 'the subject matter of many portions of Scripture must have been supernaturally revealed, while,... other details of the sacred history have been derived from natural sources.' Everything, then, in Scripture is written under the guidance of the Spirit (i.e. 'inspired') but not everything is revealed by God.<sup>25</sup>

Coleridge makes a similar distinction, though he develops it differently. Lee, whilst recognising that natural means are used, infers from the Spirit's inspiration that no errors have in fact been made. Coleridge does not make such an inference.

Lee, like Simeon and most moderate Evangelicals, recognises that the Holy Spirit does not use mechanical dictation but:

'The actuation of the Spirit will not consist in the exclusion of the Human element, but rather in illuminating and exalting it, according to its several varieties, for the attainment of the end proposed.'<sup>26</sup>

Barth also refers to Coleridge's attacks in his letters on the practice of tearing verses out of their contexts without regard to place or dispensation, in order to create 'some new credendum.'<sup>27</sup> This criticism, surely, would have been entirely acceptable to moderate Evangelicals.

Neither would Evangelicals have necessarily been 'shocked' at Coleridge's suggestion that the Bible should be read 'like any other book', especially when he added:

'... the more tranquilly an inquirer takes up the Bible as he would any other body of ancient writings, the livelier and steadier will be his impressions of its superiority to all other books.'<sup>28</sup>

Why should any Evangelical object to this ? It could be taken simply to mean that reading the Bible alongside and in the same 'objective' manner as reading other ancient literature would convince anyone of its superiority. But Coleridge went beyond this approach, for example in saying:

'In my last letter I said that in the Bible there is more that finds me than I have experienced in all other books put together, that the words of the Bible find me at greater depths of my being; and that whatever finds me brings with it an irresistible evidence of its having proceeded from the Holy Spirit.'<sup>29</sup>

Though often quoted to evidence Coleridge's radicalism, even this passage could be taken as orthodox. Coleridge's contemporary critics, however, took it to imply a subjectivism not simply in arriving at the conclusion that the Bible was inspired (quite an acceptable context), but in deciding which parts of it were 'revealed' and which were not. Coleridge's language about parts 'finding' him does not, of course, simply refer to the necessity of the Holy Spirit illuminating and applying Scripture to the individual. Both Wesley and Simeon would have agreed that this was necessary.<sup>30</sup> Coleridge was taken to be subjectively validating piecemeal. Thus e.g. Rigg (1857):

'Coleridge's view of inspiration, as implied throughout his writings, and explicitly stated in his posthumous work, "The Confessions of an Inquiring Spirit," is that the Bible is only so far inspired to any man as it brings the voice of God home to his heart, and "finds him at his lowest depths." In this sense he denies, in his "Notes on English Divines," that there can be any "revelation ab extra."

Throughout it is implied that only so far as the Reason in us

answers to and authenticates the contents of the Bible, can they be considered as entitled to the authority of the Word of God.<sup>31</sup>

Even if Coleridge concluded, said Rigg, that all the Bible was correct in point of fact, he was still judging each part by reason. Moreover, the Bible was then no more inspired than any other book (e.g. Wesley's hymns) which might 'find' people.

Barth records how the sympathetic Shedd, editing Coleridge's works in the same year (1857), vacillated over whether Coleridge was finally a 'subjectivist' in the fashion which Rigg clearly thought he was.<sup>32</sup> Tulloch (1885) takes Coleridge to be suggesting

subjectivist criteria for determining which parts of Scripture have 'Divine authority' and which do not.<sup>33</sup> Coming into modern times, most scholars have emphasized Coleridge's subjectivism.

Thus Sanders (1942) presumes that in Coleridge's view 'The Bible was not one book, but a collection of books, some of which were more faithful to the message of the Holy Spirit than were others.'<sup>34</sup>

McDonald (1959) sees Coleridge's approach as highly subjective, making specific comparison with Karl Barth and Brunner.<sup>35</sup> J.R.

Barth (1969) is critical of this, specifically referring to McDonald as having exaggerated Coleridge's subjectivism.<sup>36</sup> In the same year (1969), however, McFarland was also making comparisons of Coleridge with Karl Barth.<sup>37</sup> Reardon (1971) again sees

Coleridge's approach as subjective.<sup>38</sup> Prickett (1976) sees Coleridge as more radical in rejecting certain parts of Scripture as literal than he dared to show in print.<sup>39</sup> Pym (1978) found 'an inconsistency in Coleridge's understanding of the Bible: 'His interest in Higher criticism could not easily be wedded to a

view of inspiration which allowed Scripture to be evaluated by a subjective process of the mind.'<sup>40</sup> The 'inconsistency' perhaps appears in a different guise when Cooke (1979) sees as a statement of Coleridge's 'mature position' the identical passage which had been seen by R.J.Barth as one where Coleridge was 'careless or incomplete in making his position clear.'<sup>41</sup>

There is some evidence in Coleridge that he gave some kind of indication as to the kinds of passages which had definite divine authority. Thus Barth says of Coleridge that he accepts as unquestionably revealed (and hence infallibly true) 'whatever is referred by the sacred Penman to a direct communication from God...'<sup>42</sup> Cooke takes him to believe that only the Pentateuch can, on the evidence of the Bible itself, be described as the word of God, directly imparted to man. The rest of the Bible, written by holy men under God's influence, is rich in spiritual aid but cannot be taken for literal truth (except where we are expressly told in the Bible that the words are the word of God).<sup>43</sup> The general feeling, then, seems to be that there is a tension within Coleridge. He wants to preserve at least some parts as objectively true and revealed. But at the same time his overriding concern is that any truth of Scripture should be 'self-authenticating'. It seems that finally his view comes down to a subjective approach.<sup>44</sup>

Before looking at Sedgwick himself, we should note that in 1835-6 D.F.Strauss's The Life of Jesus Critically Examined was published. This was a very influential version of what has been called 'view (d)' above. Strauss regards all miracles (in the manner of Hume) as impossible. They are 'myths', but



for Strauss, myth expresses or embodies various ideas which the early Christians had about Christ. There are 'pure myths', which have no historical foundation at all - like the feeding of the 5000 - which grew out of belief in Jesus' messiahship.<sup>45</sup> Then there are 'historical myths' in which an actual historical event has been given a 'miraculous' symbolic scene - such as the miraculous draught of fishes which embodies Jesus' teaching about being fishers of men.<sup>46</sup> Strauss' ideas led into a programme of 'demythologizing' - of seeking to reinterpret the story of Jesus in 'modern' terms, having removed the outward form of myth in which it was wrapped by the first Christians. It also led to a 'quest for the historical Jesus' - a non-miraculous kind of teacher of righteousness and truth. The focus switched from the acts of Jesus largely to his teachings. This is not the place to comment on the success or otherwise of the movement or of Strauss himself.<sup>47</sup> But orthodox Christians in the 1840's eyed such ideas with total abhorrence. The Coleridgeans (and other liberals like Thirlwall) had a rather more complex relationship to the ideas. They shared some of the assumptions made in the critical historical methodology of Germanic scholarship, but thought that Strauss had not applied it well. Clearly, neither Coleridge nor his followers were anything like as radical as Strauss. Nevertheless, Coleridge's attitude to (say) miracle has some resemblance. In placing the accent on the observer rather than the actual act, he left it open to suppose that there was nothing real about the act itself - and a conclusion that for modern consumption the accounts would be best 'demythologized'.

### 3.1.4 Sedgwick on Inspiration

Turning to Sedgwick, we find that his general language about the Bible is Evangelical in tone. It is God's 'infallible Word',<sup>48</sup>

and in that sense all his letters and works refer to it. He says, moreover:

'If all our faith be not a mere mockery and delusion, we must be led by it to drink daily at the fountain of **sacred truth** - to study the Word of God - to accept it as teaching us the means of grace and the rule of life.'<sup>49</sup>

What was Sedgwick's attitude to inerrancy? It must first be noted that he virulently opposed and detested 'Strauss and all the lesser fry of his school'.<sup>50</sup> The Bible was basically historical and (as we shall see) Sedgwick rejoiced in the works of Paley and John Smith showing that it was. Sedgwick did not have any kind of sophisticated view of observation and truth. Strauss' Life of Jesus shews us 'the extravagance of Rationalism pushed into the form of Pantheism.' The older and new Rationalists are compared:

'The Older Rationalists admit the historical character of the Gospel, but (on principles like those of Hume) deny its miracles. The modern or atheistic Rationalist turns the whole Gospel into a myth or poetic dream; and yet some of this school pretend, in their own mythical sense, to admit the miracles of the Gospel as symbols of the development of the human mind.'<sup>51</sup>

Sedgwick thinks it unimportant which set of Rationalists win their present argument - both amount to atheism anyway. Strauss elevates human nature, rather than 'prostrating it to the dust' as does Christianity, and Strauss can never be a Christian in such a frame of mind. Any impartial student must conclude that the Gospel histories are 'artless, earnest and essentially true', and Sedgwick recommends Paley's Evidences and Horae Paulinae as demonstrations

of their essential accuracy.<sup>52</sup>

On the question of inerrancy we might here distinguish historical from scientific issues. On historical issues, Sedgwick reveals something of his attitude in a note to the section where he is virulently attacking Strauss' idea of myth. Some of the apparent discrepancies Sedgwick sees in fact as a pledge of the veracity of the accounts, for they 'prove that the primitive Christians were too honest, and had far too great a regard for the sacred text, to change so much as one word of it in the view of bringing the Gospels into a more literal and verbal accordance.'<sup>53</sup> His note refers to two claimed discrepancies: the dating of the Roman census recorded by Luke, and the different timing of the Crucifixion by John and the Synoptics. Sedgwick says:

'I will dismiss all the explanations of these difficulties, and suppose (for the sake of argument) that St Luke made a chronological mistake in stating a known fact of Roman history - and that St Mark (who was probably not an eye witness) made a mistake in describing the hour when our Saviour's crucifixion took place. Do we, then, invalidate the truth of the Gospel-history, or its inspiration, by any such concession? not, I think, in the least degree. Inspiration implies an honest, truth-loving, spirit on the part of the sacred writers; and implies also a knowledge of religious truth and doctrine derived from supernatural and divine authority; but it by no means implies, that in matters of common history, or in events about which an Apostle could (like any other man) judge by the plain evidence of his natural understanding and his senses, he had any positive inspired guidance. If we hold a contrary opinion, and demand for the sacred writers nothing short of

plenary inspiration, we become involved in most formidable difficulties, and virtually undermine the authority of miracles, which first appealed to the natural senses for their evidence. The discrepancies, here noticed, have no bearing on any religious doctrine or sectarian opinion; nor can the shadow of any ground be shown why an Evangelist should have wished to misrepresent the facts out of which they have arisen.<sup>54</sup>

Several things may be noticed about this. First, Sedgwick's argument is that mechanical dictation would negate the value of miracles as evidence - for then God could presumably have dictated something which did not happen. This has a certain validity - but inerrancy need not imply dictation. As we have seen, Lee would have fully accepted that Luke and Mark used their own resources to determine points of known history - but 'inspiration' to him meant that the Spirit would have so guided them in their researches that they would not, in point of fact, make any mistakes. Leading Evangelicals could, however, sometimes express themselves in ways bearing some resemblance to Sedgwick. Consider the following:

'Inspiration, in my opinion, was of two kinds, according to necessity, yet ever sufficient to preserve truth:- plenary inspiration, to reveal those things which man could not know, of which the writer did not know: supervisory inspiration, to watch over the things which the writer did know and to prevent him from going wrong. God did not change a writer's character: if of poetic genius, his writing was poetic; if prosaic and plain, such also was his writing. Nay, perhaps some things might be allowed in the writer which

are like error: thus one would give one order of minute events, and another give another, for this is the fact as we see it. But whenever anything depended on chronological arrangement, then there will be found a perfect agreement.<sup>55</sup>

Simeon, as reported here by Abner Brown, certainly seems to go a long way. Even to say that there were things which 'seem like error' is remarkable in so prominent an Evangelical as Simeon. Yet it is still a step from an 'unimportant' alteration in chronology, to an 'unimportant' detail of timing or of dating by contemporary history. Sedgwick's step would not have been acceptable to mainstream Evangelical leaders like Simeon, who believed in 'inerrancy' through the supervisory activity of the Spirit.

But a second point to make from the above note from Sedgwick is that it is unclear how far he himself accepts the reality of the discrepancies. He supposes 'for the sake of argument' that they are real, and proceeds to show that even if they were this would not invalidate the basic truth of the Gospels. Even supposing that there were minor discrepancies - we should not have to resort to concluding that the Bible was full of myths. But how far Sedgwick himself believed that the discrepancies were real rather than apparent is not clear from the passage. He diverges from the usual Evangelical position in being prepared to admit the possibility but does not commit himself as to the actuality.

Thirdly, whilst Sedgwick's view is less strict than the usual Evangelical one, it has no hallmarks of what is typically Coleridgean either. There is no appeal to the subjective, to what 'finds' me,

and so forth. The permissible errors are restricted to what is minor and incidental and affects no doctrine. Nor is there any tendency to play down miracles - in the manner of some of the other (particularly later) liberals. One argument against mechanical dictation is that it negates the evidence of miracles.

On scientific issues Sedgwick says:

'But if the Bible be a rule of life and faith - a record of our moral destinies - it is not (I repeat) nor does it pretend to be, a revelation of natural science.'<sup>56</sup>

Now this could be taken in two ways. As the British Critic commented around this time: 'We must beware of confounding the very dissimilar propositions that the main object of the Bible was not to teach physics or that it teaches false physics.'<sup>57</sup> The traditional Baconian approach spoke of the book of God's works and the book of God's Word - and said that neither could err. This is exactly the kind of interpretation which Sedgwick intended of his words, and the conclusion he draws (as indeed much of his Discourse) is couched in highly Baconian terms. He gives no indication that the science in the Bible might actually be in error, and comments later:

"That there are difficulties in the interpretation of the opening words of the Book of Genesis, we do not deny. To bring them into a literal accordance with all the phenomena in the past history of Nature would imply, on our part, a perfect knowledge of the past history of Nature; but such a knowledge we have not. The progress of science may clear up these difficulties...'<sup>58</sup>

This was always his reaction - saying that we do not yet have sufficient knowledge to attempt the reconciliation. But what is

significant is that he accepted the need to seek such a reconciliation. Surely neither Coleridge nor the later Powell would have thought it a necessary thing to do?<sup>59</sup>

There is, on this issue as on a number of others, a close parallel between the views of Sedgwick and the Evangelical Hugh Miller. Miller also refers to Bacon's two books and of 'alleged discrepancies between the terms of Biblical history... and the positive evidence of science.' He says: 'All such discordances, whether real or apparent, will find the proper means of adjustment readily and finally in due time.'<sup>60</sup> Miller, unlike Sedgwick, later became more optimistic that enough information had become available to actually make that adjustment, but the spirit of this is the same.

Now Cannon asserts:

'Reconciling Genesis and geology was a hobby for some Victorians, but it was openly renounced by the Catastrophist scientists.'<sup>61</sup>

His footnote then cites Sedgwick as an example. But his comment is much too strong. Sedgwick did not openly 'renounce it'. He openly repudiated Scriptural Geologists who used the Bible to construct their geology - but did not repudiate attempts to reconcile them. Indeed he thought reconciling them important, but that insufficient evidence was yet available to do so.

The tenor and framework within which Sedgwick approached the Bible was very similar to mainstream moderate Evangelicals. He does not show any signs of the distinctively subjectivist Coleridgean approach, nor of a liberal rejection of the miraculous in the Bible. On the other hand his view of what is essential to the doctrine of inspiration of Scripture does not include strict

inerrancy - even though his own personal position on inerrancy is not made clear. This is virtually the single point on which Sedgwick's religion is seen to diverge from mainstream Evangelicalism. Ironically, he never seems to have applied it in the area of science, and on scientific issues his views were virtually identical to the kind of line accepted by Chalmers and Miller and the Christian Observer.<sup>62</sup> Certainly the latter organ never saw fit to alter its view in 1834:

'we could wish that even Professor Sedgwick had been more guarded than he is in some of his expressions, though we do not believe for one moment that he meant to disparage the Divine inspiration, and consequent unerring truth of any syllable of the Divine word. The Bible is as true in its physical as in its moral affirmations.'<sup>63</sup>

Sedgwick never did seek to escape any apparent scientific difficulties by a Powellian stratagem of saying the Bible was accurate only on moral or spiritual issues. As the Christian Observer argues, physical statements do tell us of God's relationship to his world and us in it, and no such distinction is possible. Sedgwick is neither setting up a subjective selection procedure, nor a radical division, but referring only and specifically to historical details which could have no bearing on doctrine.



### 3.1 Notes

1. R. Hooker, Works, 2, p. 383.
2. Historically in the nineteenth century this was associated with the 'Recordites', with 'Fundamentalism' from the 1920's and it lives on today amongst some of the so called 'Creationists'. It seems to thrive particularly amongst Calvinists.
3. This has perhaps been the majority view amongst moderate Evangelicals since the beginning of the nineteenth century, though sometimes the docetic view seems to gain ground.
4. This is largely associated with those who would call themselves 'liberal', but the division is not clearcut. Recently a leading English Evangelical, David Winter (ex-editor of Crusade magazine) advocated something similar in But This I can Believe.
5. The obvious modern movements which may be associated with this are the Christian Existentialism springing from Dostoyevsky and Kierkegaard, and the neo-liberalism of Barth and Brunner.
6. Bultmann's name has been much associated with this in the twentieth century, though John Robinson's Honest to God created more stir at a popular level in the 1960's.
7. A.L. Lilley, Religion and Revelation, p. 79.
8. F.R. Barry, The Relevance of Christianity, p. 24.
9. B.B. Warfield, The Westminster Assembly and Its Work, p. 262.
10. J. Owen, Works, 16, p. 305.
11. Conybeare, 'Church Parties' (see part 2.3.1 above) p. 81. Conybeare, however (like many liberals, and some modern Coleridge commentators)

confounds together view (a) and view (b).

12. Thackray and Morrell, Gentlemen of Science, p. 234.
13. A Clear and Concise Demonstration of the Divine Inspiration of the Holy Scriptures.
14. Interestingly, on some 'scientific' issues which have sometimes been expected to be settled from Scripture, Wesley was content to accept the evidence of nature. An example of this is the issue of spontaneous generation (see his Journal entry for 21st June 1758).
15. Works, 17, p. 497.
16. Works, 14, p. 216.
17. Works, 19, p. 72.
18. H.E. Hopkins, Charles Simeon, p. 177; also cited A. Pollard and M. Hennell, Charles Simeon (1759-1836), p. 44.
19. See below section (6).
20. Chalmers, Preface to the Institutes of Theology.
21. John Pye Smith, Memoirs, p. 285.
22. Ibid., p. 285.
23. All are recommended during Smith's The Mosaic Account of the Deluge Illustrated by the Discoveries of Modern Science (1838).
24. A Letter, dated 26th April 1822, in W. Carus, Memoirs of the Life of the Rev. Charles Simeon, pp. 562-568 (quotation on p. 567-8).
25. J.R. Barth, Coleridge and Christian Doctrine, p. 38.
26. W. Lee, The Inspiration of Scripture, p. 38.
27. Barth (ref. 25), p. 67.
28. Confessions of an Inquiring Spirit, p. 75. K. Cooke (Coleridge) cites this on p. 197 and says: 'even today this would offend some,

and in 1830 could be relied upon to cause disquiet in many orthodox Christians'. Since none are actually cited who expressed any disquiet, I am reduced to the comment that speaking personally as an Evangelical I find it unexceptional. M.M. Garland, Cambridge Before Darwin, (p. 86), is misleading in citing Coleridge's advice to read the Bible like any other book - Coleridge did not deny its special nature.

29. Confessions of an Inquiring Spirit, p. 43.
30. Wesley stresses that the Spirit is needed to make the revelation of the Bible effectual inside the individual e.g. in Awake Thou That Sleepest (Works, 5, p. 22-31) he stresses that the question is "Hast thou the witness in thyself?" (see also sermon on pp. 104-125). Simeon emphasized that without the inner working of the Spirit the Bible is a 'dead letter' e.g. Works, 5, p. 373.
31. J.H. Rigg, Modern Anglican Theology, p. 18; Lee (ref. 26) also attacks Coleridge in various footnotes on inerrancy.
32. Barth (ref. 25), p. 69.
33. J. Tulloch, Movements of Religious Thought in Britain during the Nineteenth Century, p. 26.
34. C.R. Sanders, Coleridge and the Broad Church Movement, p. 82.
35. H.D. McDonald, Ideas of Revelation, p. 174.
36. Barth (ref. 25), p. 68.
37. T. McFarland, Coleridge and The Pantheist Tradition, p. 318, etc.
38. B.M. Reardon, Religious Thought in the Nineteenth Century, p. 82  
'The final testimony, as always, is that of personal experience.'
39. S. Prickett, Romanticism and Religion, p. 47.
40. D. Pym, The Religious Thought of S.T. Coleridge, p. 82.

41. K. Cooke, Coleridge, p. 199, Barth (ref. 25), p. 69n.
42. Barth (ref. 25), p. 61 is citing Coleridge's letters in E.L. Griggs, The Collected Letters of Samuel Taylor Coleridge, 3, p. 589; J.D. Boulger, Coleridge as a Religious Thinker, p. 187, cites the Confessions (p. 585) to the same effect.
43. Cooke (ref. 41), p. 196. Yet Prickett (ref. 39), p. 47 records Coleridge's doubts about the historicity of the flood.
44. McDonald (ref. 35), p. 179-194 goes on to look at the views of Hare and Maurice, and to give a critique of all the Coleridgeans from an Evangelical viewpoint. It is controversial how far Hare and Maurice did actually follow Coleridge on inspiration, but it is not sufficiently important to our theme to pursue here.
45. See D.F. Strauss, The Life of Jesus Critically Examined, pp. 39-94. Any narration is presumed myth if it 'is irreconcilable with the known and universal laws... according to these laws, agreeing with all just philosophical conceptions and all credible experience, the absolute never disturbs the chain of secondary causes by single arbitrary acts of interposition...' (p. 88). No wonder Sedgwick, in the English tradition and as a Christian and scientist, took exception to this as a starting point.
46. Ibid., p. 507, p. 503, etc.
47. A useful modern critique from a conservative point of view is that of J.D.G. Dunn in I.H. Marshall, New Testament Interpretation, ch. xv.
48. 5th Edition Discourse, cccxix.
49. Ibid., cccxx.
50. Sedgwick consistently attacks Strauss, e.g. 5th Edition Discourse, and letter in Clark & Hughes to John Smith dated 21st September 1855.

51. 5th Edition Discourse, cclxiii.
52. Ibid., cclxxiv-cclxxv.
53. Ibid., cclxi.
54. Ibid., cclxi - cclxii.
55. W.A. Brown, Recollections of the Conversation Parties of the Rev. Charles Simeon, p. 367.
56. 1st Edition Discourse, p. 104 and all editions.
57. British Critic, 1834, 30, p. 413.
58. 5th Edition Discourse, cccvi.
59. Strangely, Coleridge came increasingly to see the first chapter of Genesis as a kind of exception to the general rule that the Bible was not to teach science, and to regard it as 'providing certain truths a priori'. (see H. Levere, Poetry Revealed in Nature, p. 135 etc.). But this is the other way around from Sedgwick. Coleridge had no theological framework of belief in inspiration which would make him expect scientific compatibility - but through his study of science he came empirically to believe that it did have. Sedgwick, on the other hand, found problems through what he actually discovered in science, but because of his theological framework of belief in inspiration had a prior religious expectancy that ultimately the two would prove to be compatible.
60. H. Miller, First Impressions of England and Its People.
61. W.F. Cannon, 'The Uniformist Catastrophist Debate', Isis, 1960, 51, 38-46.
62. These issues will be looked at in sections (5) and (6).
63. Christian Observer, June 1834, p. 375.

### 3.2 Reason, Revelation and Faith

#### 3.2.1 Christians and Reason

The relationship between reason, inner experience, external evidence, and revelation in various traditions of theology are complex and varied. Respective pairs can be seen either as complements or as substitutes. The Deists, for example, saw reason and evidence as alternatives to revelation, sometimes virtually reducing religion to belief in God plus morals.<sup>1</sup> Others were critical of so high a view of reason, though none could deny some role for it. Of Evangelicals, Wesley, for example, rejected both the subjectivism of Dodwell and the Deism of Tillotson.<sup>2</sup> Reason, to Wesley, was neither all nor nothing: 'It is a fundamental principle with us that to renounce reason is to renounce religion, that religion and reason go hand in hand...'<sup>3</sup> Reason was a precious gift of God, and the 'candle of the Lord',<sup>4</sup> but cannot create faith, hope and love of itself. The understanding of faith comes when God enlightens our minds 'and we then see, not by a chain of reasoning but by a kind of intuition, by a direct view.'<sup>5</sup> This was actually nearer than many have supposed to men like Butler and Paley.<sup>6</sup>

In Sedgwick's own generation, it will be useful to compare his views on reason with those of some recognised leaders of groups, such as Simeon, Chalmers, Coleridge and Newman. In the present chapter, however, we may leave issues more specific to the 'evidences' and natural religion to be dealt with separately.

Simeon accepted reason if assigned a moderate role. He wrote 'The only use of reason as applied to revelation is to ascertain whether the revelation, purporting to be from heaven, be indeed of Divine authority, and what is the true import of that revelation in all its parts.'<sup>7</sup> But the content of that revelation is not irrational: 'though revealed religion is neither founded

on human reason, nor makes its appeal to it; yet it is perfectly consistent with reason and approves itself to the judgement of everyone whose mind is enlightened by the Spirit of God.'<sup>8</sup> We may note, however, the last phrase, for man's fall and the resulting corruption has made natural reason unreliable in spiritual matters (even though it may be competent in areas like science).<sup>9</sup> Spiritual understanding was not the same as scholarly knowledge.<sup>10</sup> Simeon often preached on 'wisdom', which he distinguished from learning. But he even says: 'Intellect is that which distinguishes man from the brute creation- and the enlargement of it with arts and sciences is that which elevates man above his fellows...'<sup>11</sup>

Thomas Chalmers' approach was not unlike Simeon's. In 1817 he wrote:

'We do not condemn the exercise of reason in matters of theology... Reason can judge of the external evidences for Christianity, because it can discern the merits of human testimony: and it can perceive the truth or the falsehood of such obvious credentials as the performance of a miracle, or the fulfilment of a prophecy. But reason is not entitled to sit in judgement over those internal evidences which many a presumptuous theologian has attempted to derive from the reason of the thing, or from the agreement of the doctrine with the fancied character and attributes of the Deity...'<sup>12</sup>

Chalmers objects to the judgement of the Bible's doctrines by a priori ideas of necessary attributes of God, in the style of Samuel Clarke.

On the more basic question of Faith vs Belief, clearly no Evangelical would have thought mere belief sufficient, and both Simeon and Chalmers emphasized active faith.

The Coleridgeans drew, like Wesley, from the Cambridge Platonists and from Law, but also from Spinoza, Kant, and the German traditions. Care must be taken in making any comparison with Evangelicals, since Coleridge used words like 'reason' in a technical sense, whereas so far in this chapter the word has been used in a more normal sense. Coleridge's more sophisticated philosophical analysis distinguished various kinds of reason - following but not identical to the scheme of Kant. Kant, in his Critique of Pure Reason, called 'understanding' the 'faculty of cognition through concepts'. Concepts were a means used in the mind to interpret sensory experience. He distinguished this from 'Reason', which concerned Ideas transcending experience in the sense that no objects can be given within experience which correspond to them. They are Ideas arising in the human mind's tendency to seek unconditioned unifying principles - not deducible from experience. In The Critique of Practical Reason Kant elaborated on a further distinction between Pure Reason (or Speculative Reason) which is concerned with objects of the cognitive power (science etc.), and Practical Reason which concerns Ideas involved in determining the will and the principles of moral conduct. Kant's religion (cf Religion Within the Bounds of Reason) was based on a cold categorical imperative, and Colin Brown has gone as far as to call it a 'religion without God.'<sup>13</sup> Coleridge's adaptation of this adds a massive 'God consciousness'.

Coleridge distinguished three powers in the human mind - sense, understanding, and reason - and in every act of the mind the three united.<sup>14</sup> Understanding is a power shared with animals but transformed by its unique combination in man with reason.<sup>15</sup> It is a power to receive impressions and sensations, a perception of



the physical world of space and time leading to generalisation from this empirical data.<sup>16</sup> On the other hand:

'It is the Office, and as it were, the instinct of the reason, to bring a unity into all our conceptions and several knowledges. On this all system depends; and without this we could reflect connectedly neither on nature nor on our own minds. Now this is possible only on the hypothesis of a One as the ground and cause of the universe, and which, in all succession and through all changes, is the subject neither of time nor of change. The One must be contemplated as eternal and immutable.'<sup>17</sup>

Like Kant, Coleridge distinguishes two forms of Reason:

'Contemplated distinctively in reference to formal (or abstract) truth, it is the Speculative Reason; but in reference to actual (or moral) truth, as the fountain of ideas and light of the conscience, we name it the Practical Reason. Whenever by self-subjection to this universal light, the will of the individual, the particular will, has become a will of reason, the man is regenerate: and reason is then the spirit of the regenerated man, whereby the person is capable of a quickening inter-communion with the Divine Spirit.'<sup>18</sup>

Coleridge's 'Speculative Reason' seems the same as Kant's - but his 'Practical Reason' goes far further. To Kant it was a moral felt need, but primarily was a detached realisation that the categorical imperative (somehow self-evident - though this is one of the least convincing parts of Kant) led to ethical obligations. God is introduced as a convenient hypothesis to give unity to the ethical system. But to Coleridge, Practical Reason does imply the recognition of a moral need, but also enables an actual experience

of God to commence in a personal relationship. Bringing the individual will into line with Divine will brings release and enlightenment. Kant might be regarded as a species of Deist, in whose system the Christian doctrines of Jesus and Redemption had no structural place. Coleridge, with the centrality of his philosophical trinitarianism (albeit bearing the slur of pantheism) rethought Kantian concepts and approaches in a profoundly Christian way - and his God is an acting and reacting Being.

But Coleridge's change in the meaning of 'Practical Reason' does lead to some confusion. In one place Coleridge insists that all men possess Reason equally.<sup>19</sup> But communion with God depends upon an act of Will - in response to the Divine drawing - so:

'all the organs of sense are framed for a corresponding world of sense; and we have it. All the organs of spirit are framed for a corresponding world of spirit; though the latter are not developed in all alike. But they exist in all, and their first appearance discloses itself in the moral being.'<sup>20</sup>

It would not be within the purpose of this thesis to examine the various arguments surrounding Coleridge's use of the Kantian concepts of Reason,<sup>21</sup> nor how far 'Practical Reason' is to be identified with faith. But enough has been said to show the care that needs to be taken in comparing statements about 'Reason' made by Evangelicals and Coleridge. They are not using the same language.

It needs, however, to be remembered that they had certain things in common. Both were reacting against a purely Rationalist analysis of (and rejection of) God. Both were rejecting the materialism of Hume and the descendants of Locke. This is true of the Romantic movement generally. Sedgwick, of course,

was introduced to the Lake poets through the Evangelical Clarksons.<sup>22</sup> But, more deeply, we find John Keble, the High Church poet much beloved of Evangelicals for his The Christian Year, recognising his affinities with Wordsworth.<sup>23</sup> The Tractarians, too, had a common enemy in the Materialists and the Rationalists.

To Newman, also, reason had a place but was not enough. In 1839 he says it is usual at this day:

'To say that Faith is a moral quality, dependent upon Reason - that Reason judges both of the evidence on which Scripture is to be received, and of the meaning of Scripture; and that Faith follows or not, according to the state of the heart; that we make up our minds by Reason without Faith, and then we proceed to adore and obey by Faith apart from Reason; that, though Faith rests on testimony, not on reasonings, yet that testimony, in its turn depends on Reason for the proof of its pretensions, so that Reason is an indispensable preliminary.'<sup>24</sup>

But Newman protests:

'Will any one say that a child or uneducated person may not savingly act on Faith, without being able to produce reasons why he so acts ? What sufficient view has he of the Evidences of Christianity ? What logical proof of its divinity ? If he has none, Faith, viewed as a habit of the mind, does not depend upon inquiry and examination, but has its own special basis, whatever that is, as truly as Conscience has. We see, then, that Reason may be the judge, without being the origin of Faith, and that Faith may be justified by Reason without making use of it.'<sup>25</sup>

To Newman, no less than the Evangelicals or Coleridge, head knowledge was neither a necessary nor sufficient condition for Faith, for Faith involved a relationship. But Newman, of course, speaks of that relationship as a 'habit of mind' in line with his (by then) High Church convictions and approach.

### 3.2.2 Sedgwick on These Issues

Sedgwick believed all religious knowledge to come ultimately by God's self-revelation:

'... we know him only as he has thought good to reveal himself, by the law written in the heart - by the laws of the material world - and by the declarations of his word...'<sup>26</sup>

We have already seen, however, that Sedgwick had a high view of the Biblical revelation, and will further see in subsequent chapters that he by no means saw natural theology as an adequate alternative to Scripture. As for interpreting Scripture, Sedgwick expects us to use our reason:

'Our classical studies help us to interpret the oracles of God, and enable us to read the book wherein man's moral destinies are written, and the means of eternal life are placed before him.'<sup>27</sup>

Though he recognised the evidence of conscience, Sedgwick himself does not speak in the terms of his Quaker friends about the inner light. The role of the Holy Spirit is not emphasized in this.

On the associated question of Faith vs Knowledge Sedgwick says:

'There is a belief of the heart as well as a belief of the understanding. If a man have the second, and not the first, he may be a good logician and a learned theologian, but he has no religious faith. If a man have the first and scorn the second, he is a fanatic. But a good religious man has both.'<sup>28</sup>

Two things here are apparent. First, Sedgwick is clear that belief is not the same as faith, and would have totally rejected not just Tindal, but Browne. Secondly, he would have equally firmly have rejected Dodwell - real faith involves the reason and understanding. Sedgwick recognises, of course, that a person may come to faith without having worked out the reasons for it.<sup>29</sup> But he recommends that Christian men, such as those he addresses at the University, should seek to understand those reasons. In doing so he recognises that philosophy in itself is not much use against temptation, and has already in the Discourse recommended one of Chalmers' sermons on The Expulsive Power of a New Affection.<sup>30</sup>

The balance of Sedgwick's approach is very similar to that of other Evangelicals of his period. His analysis lacks any of the sophistication of a Coleridge, and he uses the terms in an ordinary sense - much as Chalmers might have done.

In further chapters and sections we may now explore more specifically Sedgwick's views on the use of Christian evidences (both from Nature and Historical ones), in relation to views of others in his times.

### 3.2 Notes

1. See e.g. Tindal's Christianity as Old as the Creation (1730)
2. See Wesley, Works, 8, p. 14 for reference to Dodwell's book.  
Whitefield actually wrote against Tillotson, and H.D. McDonald (in Ideas of Revelation, p. 40) states Wesley took a similar line in unpublished papers.
3. Wesley, Selected Letters, number 126.
4. The Case of Reason Considered, Works, 6, pp. 331-341 (see p. 340)
5. The End of Christ's Coming, Works, 6, pp. 251-260.
6. See e.g. R.E. Davies, Methodism, p. 75f., A. Whyte, Bishop Butler, p. 86f.
7. Works, 16, p. 121.
8. Ibid., 16, p. 229.
9. Ibid., 14, p. 53.
10. Ibid., 7, p. 507.
11. Sermon 830.
12. The Evidence and Authority of the Christian Revelation, p. 275-6.
13. C. Brown, Philosophy and the Christian Faith, p. 105.
14. Coleridge, Friend, p. 164.
15. Aids to Reflection, p. 261.
16. Ibid., p. 251f.
17. Ibid., p. 210-1.
18. Ibid., p. 241-2.
19. Friend, p. 176.
20. Biographia Litteraria, 1, p. 108.

21. See e.g. J.H. Muirhead, Coleridge as Philosopher, ch. 2; J.D. Boulger, Coleridge as a Religious Thinker, ch. 3; B.M. Reardon, From Coleridge to Gore, p. 63f; K. Cooke, Coleridge, p. 191.
22. Clark and Hughes, 2, p. 427.
23. See especially S. Prickett, Romanticism and Religion, ch. 4.
24. Cited by Chadwick in The Mind of the Oxford Movement, p. 73.
25. Ibid., p. 75.
26. 1st Edition Discourse, p. 53 (and subsequent Editions).
27. 1st Edition Discourse, p. 31 (and subsequent Editions).
28. 5th Edition Discourse, p. ccii.
29. Ibid., Sedgwick distinguishes the logical from the historical order of our beliefs.
30. 1st Edition Discourse, p. 48 (and subsequent Editions).

### 3.3 Christian Evidences

#### 3.3.1 The Eighteenth Century Background

One of the key ideas in Christian epistemology concerns the role of 'evidences' for the truth of Christianity. Strictly, these include some of natural theology - but this aspect is important enough to need treatment in a separate section of its own. The present chapter relates to those evidences which might broadly be called 'historical' - evidences for the general accuracy of the Bible and the Divine authority of its message. Wesley, for example, was one of many who appealed to the classic proofs of miracles, prophecies, and the moral character of its writers and teachings, to support Biblical authority.<sup>1</sup> But by the late eighteenth century the Deists had raised questions about its historicity, and attacked the traditional proofs from miracles exactly because miracles were incredible and against their conceptions of natural philosophy.

Against this background, Paley made his classic statements of the Christian evidences in 1790 and 1794. First, he dealt with Hume's objections to miracles, arguing: 'Once believe in a God and miracles are not incredible.'<sup>2</sup> Hume, he said, began from an assumption foreign to his own sceptical philosophy, dogmatically assuming a law of uniformity of nature. On the rarity of miracles, Paley argued that if God uses them only to make some special point (e.g. authenticating a revelation) then we should expect them to be rare.<sup>3</sup> Paley also pointed to the fact that the early Christians suffered for their faith, and that they abandoned older beliefs as a result of seeing miracles. Both these point to the fact that they really did believe they had seen them. He contrasted other accounts of miracles, where the accounts are not first or even second hand, and/or they produce no change of life or belief, and/or they incur no persecution. So other



religions' claims to miracles are rejected.<sup>4</sup> Historically (In Evidences and in Horae Paulinae) he looked for corroborating evidences within the New Testament as in any other book. The second of these, for example, looked at the small incidental agreements on historical points between Paul's epistles and the Acts of Luke. He concluded that 'the Gospels' actually proceeded from the authors whose names they bear<sup>5</sup>, and Paul's epistles were genuine. In this LeMahieu says he 'anticipated the analytic methods of the Broad Church movement.. the attempt to read the Bible "like any other book".'<sup>6</sup> This comparison is in some senses misleading. Paley was concerned with evidence to convince a rational mind that the acceptance of the New Testament as authentic was reasonable. Hare and his associates were concerned with the use of historical methods in interpreting rather than authenticating.

### 3.3.2 Evangelical Leaders and the Evidences

Simeon saw the main role of reason in regard to revelation as 'to ascertain whether the revelation, purporting to be from heaven, be indeed of Divine authority, and what is the true import of that revelation in all its parts.'<sup>7</sup> Reason, then, must judge whether a revelation was genuinely divine. Once, however, it was accepted as genuine, it was our duty to receive it in its entirety, and reason must not be used to select from it. Simeon concludes, however, 'It has pleased God to give us every evidence of the truth of our religion, that the most scrupulous mind could desire. The proofs arising from prophecies and miracles, are such as to carry irresistible conviction to every candid enquirer.'<sup>8</sup> Elsewhere he refers to its moral goodness, and to the moral effects it had on believers.<sup>9</sup> Evangelicals did not deny the importance of evidences, but did not believe that

analysing or accepting evidences were either necessary or sufficient conditions for faith. Simeon distinguishes 'the theoretical religion' ('everything that is necessary to prove the truth of Christianity') and 'practical religion' ('whatever is required of those who embrace it').<sup>10</sup> But though knowledge of the evidences is neither a substitute for nor a guarantee of faith and commitment, it may be a useful stimulus to them. Simeon did not deny this, and one of his protégés and intimate friends produced a two volume set on the Christian evidences in 1828-1830.<sup>11</sup>

Chalmers followed a similar line to Simeon. His 1817 work The Evidences and Authority of the Christian Revelation looks at evidences for the authenticity of the New Testament documents very much in the tradition of Paley - and, like Paley, Chalmers cites the works of the nonconformist Lardner. To Chalmers, too, miracles and prophecy are important.<sup>12</sup> Chalmers, like Simeon, regards study of the evidences as neither a necessary<sup>13</sup> nor a sufficient<sup>14</sup> condition for faith, for faith concerns feeling, obedience and submission. This, however, is not to deny that they are a useful tool to aid and stimulate faith.

### 3.3.3 The Coleridgeans and the Evidences

If we now turn to the Coleridgeans we find a quite different attitude. Coleridge disliked the whole project of presenting Christian 'evidences', not merely because it was insufficient but because it was a positive distraction:

'I more than fear the prevailing taste for books of natural theology, physico-theology, demonstrations of God from Nature, evidences of Christianity and the like. Evidences of Christianity! I am weary of the word. Make a man feel the want of it; rouse him, if you can,

to the self knowledge of his need for it; and you may safely trust it to its own Evidence, - remembering only the express declaration of Christ himself: No man cometh to me, unless the Father leadeth him.'<sup>15</sup>

In a letter to Derwent in 1826 Coleridge says: 'when I read Paley, Watson, or indeed any one of the Evidence-mongers.- They are outrages (on) Logic, and insults on Common sense...'<sup>16</sup>

Boulger makes some suggestions for reasons behind Coleridge's vehemence towards Paley.<sup>17</sup> Boulger explains that Coleridge believed such thinkers to be arguing about the wrong material in the wrong way. 'In this class must be placed most of the Bampton and Boyle lecturers on the Evidences of Christianity and natural theology, as well as many an individual divine. The progenitor of all this activity was William Paley, the arch-enemy Coleridge appointed for himself.'<sup>18</sup>

Boulger sees Coleridge's opposition on a deeper metaphysical foundation, in the context of his rejection of Lockian materialism.

Two specific foci of the evidences theme might also be taken up. The first concerns miracles. Pym speaks of Coleridge's conviction 'that miracle is not the foundation of faith, as Paley had it, but a consequent. Now Coleridge saw miracle as both a concrete occurrence and a spiritual turning point in the present day believer.'<sup>19</sup> Barth adds that not only is recognition of a miracle as such 'a result of faith rather than its cause', but Coleridge objected to speaking of miracles as 'suspension of the laws of nature.'<sup>20</sup> Coleridge does not like any semblance of a distinction between the supposed normal sphere of providence and the sphere of the supernatural. The key to miracles is in their sign value, not in suspension of supposed laws. Pym concludes: 'Therefore what took place in Palestine two thousand years ago only becomes a miracle because

an individual in the present decides that for him it conveys an important spiritual 'truth'.<sup>21</sup> Pym may perhaps here be exaggerating Coleridge's subjectivity, and likewise on the historicity of the gospels as he states Coleridge came to believe 'that the Gospels are not essentially factual accounts, but rather statements of the significance of Jesus for the writers living some time after...'<sup>22</sup> Pym pictures Coleridge as a precursor of D.F. Strauss<sup>23</sup> which may be extreme, but certainly he had less interest in the historical nature of the Bible than most.<sup>24</sup>

Julius Hare may show the influence of Coleridge in the passage in his The Victory of Faith where he speaks of the rise of Rationalism after Luther had 'arisen with the spirit and power of Elias' to proclaim faith:

'But the progress of knowledge and civilisation produced its usual effect. The pride of knowledge bred the lust of knowledge; and the lust of knowledge pampered the pride of knowledge: and again it became a very general opinion that the belief of the understanding is one and the same thing with Christian Faith; and that this belief is to be grounded on testimony. Hence we were inundated with dissertations on the external evidences of Christianity; in which it was treated like any other historical fact, and witnesses were sifted and cross examined; but without regard to the main witness, the witness in the heart of the believer himself, in his infirmities, his wants and his cravings,- the witness along with which the Spirit bears witness in groanings which cannot be uttered. This, the only witness on which a living Faith in Christ can be established, was left out of sight: and so it is little to be wondered at if the Gospel half melted away into a system of philanthropical morality.'<sup>25</sup>

This appears to be a kind of mixture of Evangelicalism and Coleridge, fairly unique to Hare. But the suspicion of the 'evidences' is marked, and shows a different perspective from (say) Simeon or Chalmers.

F.D.Maurice writes to Kingsley in 1863:

'I am glad you can speak so respectfully of Paley's Evidences as you do in your Preface. I have a sneaking regard for him as a good, tough North of England man, not spoiled by his cleverness as a lawyer. But I have been fighting against him all my days; I cannot help thinking he has done much to demoralise Cambridge, and to raise up a set of Divines who turn out a bag infidel on Sundays to run him down, fixing exactly where he shall run, and being exceedingly provoked if he finds any holes and corners which they do not happen to know of.'<sup>26</sup>

In 1863 many of the battles had already been fought, and Maurice could afford some private magnanimity to Paley. But he had already said in the same letter:

'To me there is an overpowering evidence for the Resurrection in the concurrence of the testimony through all nature, and in my own being, that Death must have been overcome, that it cannot be my master as my downward inclination leads me to think it is - with the testimony of straightforward honest men, "We saw Him after he was risen, though we thought the thing too good to be true." But their testimony without the other could not affect me. I must cast it aside, let those who spoke it be ever so honest.'

### 3.3.4 Newman and Whateley

We have seen that the Coleridgeans, in disparaging the evidences, differed from the Evangelicals. They differed also from Newman, who in 1839 summed up what 'seems to me to be meant when persons disparage the Evidences.' The critics, he says, may feel that religious minds will embrace the Gospel anyway, and that on the irreligious the evidences are anyway wasted.<sup>27</sup> But Newman argues that the evidences are of use to some. They test the honesty of unbelievers, and help perplexed believers who may be harassed by objections by encouraging their faith. Moreover, 'even in the case of the most established Christians they are a source of gratitude and reverent admiration, and a means of confirming faith and hope.'<sup>28</sup> Not even a perfect evidence would produce a living faith, but the 'probabilities' the evidences afford encourage where faith exists.

Finally, it is interesting that although Whately has been placed by some in a 'Broad Church' with the Coleridgeans, on the 'evidences' issue they stood poles apart. Even LeMahieu (who accepts the concept of the 'Broad Church') admits that: 'Unlike many other members of the Broad Church movement, Whately felt neither alarmed nor mentally invigorated by German metaphysics and biblical criticism.' His emphasis on logic and disputation is described by Tuckwell: 'In thus removing theological belief from the domain of authority and faith to the domain of well instructed reasoning, the Archbishop probably impressed the lesson he desired.'<sup>29</sup> Whately's logical and cerebral approach (which differed as much from the Evangelicals as from Coleridge) led him to appreciate Paley's Evidences, and he published a new edition in 1859.

### 3.3.5 Sedgwick and the Evidences

We have seen that in Sedgwick's generation the Coleridgeans were

distinctive amongst all the theological parties in disparaging the evidences. What, then, was Sedgwick's attitude, and how far was he a part of a 'Cambridge Network' influenced by Coleridgean theology?

This question is the more significant because, as Garland well spells out<sup>30</sup>, Paley came to have an important place and influence in the Cambridge curriculum. Garland adds that the Coleridgeans, who disliked emphasis on the evidences, were 'not surprisingly, offended by the naive blend of Bible and Paley which passed for theology at Cambridge.'<sup>31</sup> But a naive blend of Bible and Paley would probably characterise much of Sedgwick's thinking. He says:

'I earnestly recommend the Cambridge undergraduates... to study the Evidences of Paley. His work is short and clear, appeals to the first principles of common sense and feeling, and forms an established part of our academic course. It is not, however, a very learned work, and may not on that account satisfy one whose delight is in deep learning; and it gives us none of the baseless visions of transcendentalism, and cannot therefore satisfy a Pantheist... but truth is its aim, and common sense its element, and it puts before the unsophisticated heart and reason a far more convincing weight of evidence than is found in any other English work.'<sup>32</sup>

Sedgwick goes on to recommend Paley's other work, the Horae Paulinae saying that the reader: 'will see a proof nothing short of moral demonstration that the book called "The Acts of the Apostles," is substantially a true history of facts and opinions.' Writing to James Smith in 1855, Sedgwick says that if he ever was drifting to Deism he was arrested in it by Butler 'who proved to me that there was no refuge in deism'.<sup>33</sup> He continues:

'Still there were difficulties. Had we the Bible in its

purity ? Had it not been tampered with ? Was it not therefore mythical in part ? Paley's Horae Paulinae was the first work that on such questions set my heart at rest. He proved to demonstration that the Epistles of Paul and the Acts of Luke were real historical documents and substantially true... come back to your book. I would put it side by side with the Horae Paulinae as a demonstrative proof, so far as it goes, of the historical truth of the gospel narrative. St Luke and St Paul were true men and published what they believed true. We cannot separate the historical part of their works from the theological, or the natural from the supernatural. They must stand or fall together.'

James Smith was a committed member of the Church of Scotland, and from a thorough knowledge both of nautical questions and of critical and historical sources wrote his very detailed analysis to show that the gospels were early accounts of eye witnesses to many of the events and that the details of Luke's history of St Paul's travels were reliable.<sup>34</sup> Its effect on Sedgwick was profound, and the subject of St Paul's travels formed a basis for some of his sermons.<sup>35</sup>

We have already seen (part 3.2.2) that Sedgwick did not mistake belief and knowledge for faith, and he was also clear that the signs of faith were exhibition of Christian graces - not abilities in tracing out proofs of religious truth.<sup>36</sup> Nevertheless he does assign some importance to the evidences - much along the lines of other Evangelicals of the times. He shows no sign whatsoever of any of the distinctively Coleridgean approach on the issue.



### 3.3 Notes

1. A Clear and Concise Demonstration of the Divine Inspiration of the Holy Scripture.
2. Evidences, p. 7.
3. Ibid., p. 6.
4. Ibid., p. 182.
5. Ibid., p. 179.
6. LeMahieu, The Mind of William Paley, p. 107; this statement has, in any case, to be treated with great care. The Coleridgeans were not saying that the Bible was just the same as any other book, but that (in contrast to the 'dictation' theories of inspiration) it had grown out of real historical situations and should be read as such.
7. Works, 16, p. 121.
8. Works, 14, p. 351.
9. Works, 15, p. 471f; 9 p. 5-6.
10. Works, 9, p. 5-6.
11. D. Wilson, Evidences of Christianity, 2 vols., 1823-30.
12. Chalmers, The Evidence and Authority of the Christian Revelation, p. 253, ch. vi.
13. Ibid., p. vi.
14. Ibid., p. 291.
15. Aids to Reflection, p. 294-5.
16. Griggs, The Collected Letters of Samuel Taylor Coleridge, 6, p. 538.
17. Boulger, Coleridge as a Religious Thinker, ch. 1.

18. Ibid., p.20. Boulger's point about Coleridge's attitude is valid, though we must presumably take the Bampton and Boyle lecturers referred to as being the nineteenth century ones; Paley could hardly be blamed for a series which started before he was born. On the same point see also C. Welch, Protestant Thought in the Nineteenth Century, p. 110.
19. Pym, The Religious Thought of S.T. Coleridge, p. 66.
20. Barth, Coleridge and Christian Doctrine, p. 39.
21. Pym (ref. 19), p. 67.
22. Ibid., p. 68.
23. Ibid., p. 71.
24. See also Prickett, Romanticism and Religion, p. 47.
25. Hare, The Victory of Faith, p. 17.
26. F. Maurice, Life of F.D. Maurice, 2, p. 450.
27. Chadwick, The Mind of the Oxford Movement, p. 79.
28. Ibid., p. 79.
29. B. Tuckwell, Pre Tractarian Oxford, p. 76.
30. Garland, Cambridge Before Darwin, ch. 4.
31. Ibid., p. 64.
32. 5th Edition Discourse, p. cclxxiv.
33. Clark & Hughes, 2, p. 299.
34. James Smith, The Voyage and Shipwreck of St Paul (1848).
35. Clark & Hughes, 2, p. 585.
36. See e.g. 1st Edition Discourse, p. 79.

#### (4) NATURAL THEOLOGY

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##### 4.1 The Nineteenth Century Background

###### 4.1.1 The Intellectual Heritage

The present section deals with natural theology, both in its arguments from moral sense and in its physico-theology. But first we need to recognise the long heritage in English thinking on these.

Robert Boyle became concerned that the rising (though small) number of atheists might try to see in the 'mechanical philosophy' a basis for their infidelity. In 1690, therefore, he wrote The Christian Virtuoso, arguing that a better knowledge of nature pointed rather to acknowledgement of a creator. He also founded a series of lectures (the first by Bentley in 1692) on this theme, and an extensive literature, particularly on physico-theology, developed.

The controversy increasingly centred on the Deists or semi-Deists rather than atheists, and works of Locke, Toland, Collins, and Tindal were seen as denying the importance of revelation and sometimes substituting a God-of-nature for the Biblical God. Butler, in 1736, wrote his famous Analogy, arguing that any Deist who accepted a God of nature could find nothing to object to in the Bible, since analogies always existed in nature for the truths taught in Scripture. Butler remained of great importance to Sedgwick's generation, as did that later 18th century figure William Paley. Paley's refinement of the watch analogy argued that the argument from design stood even if one had never seen a watch made and could not fully understand its working.<sup>1</sup> From nature, he argued God's omnipotence, omniscience, omnipresence, self existence, necessary existence, and spirituality.<sup>2</sup> Both Butler and Paley were basically orthodox in theology.<sup>3</sup> On most issues their respective writings were complementary, but on one

issue they were opposed. Butler had written:

'There is a superior principle of reflection or conscience in every man, which distinguishes between the internal principles of his heart as well as his external actions; which passes judgement upon himself and them, pronounces determinately some actions to be in themselves just, right, good, others to be in themselves evil, wrong, unjust...'<sup>4</sup>

Paley, however, said either such a moral sense did not exist, or else it was too affected by prejudice or habit to be reliable.<sup>5</sup>

Paley's defence of Christian ethics was purely logical, prefiguring the utilitarianism of Jeremy Bentham<sup>6</sup> by portraying the way to find God's will as an enquiry into 'the tendency of the action to promote or diminish the general happiness.'<sup>7</sup> With the exception, then, of the 'plurality of worlds' issue (which is dealt with separately below), between them Butler and Paley had stated or restated all the following issues of natural theology which interested the early nineteenth century intellectuals:

- (A) The physico-theological argument that design implied a designer.
- (B) The arguments from analogy against Deist objections to Christianity.
- (C) The natural theological arguments for the morality of God:
  - (i) (Paley) from physical design of the world providing for pleasure rather than pain (Paley tied this to his 'utilitarian' ethics).
  - (ii) (Butler) from conscience and a universal human experience.

The remainder of the present chapter considers the reactions, on these issues, of the various nineteenth century church parties (examined as previously by looking at the views of selected leading members), as a context for consideration of Sedgwick and his immediate circle. Put briefly, virtually all church parties favoured Butler rather than

Paley on point (C), though Coleridge seems more indebted to Kant than Butler for analysis of concepts like conscience. Leading moderate [evangelicals and the Newman school united in accepting (A) and (B), though none saw such rational considerations as a substitute for faith, whilst Coleridge and his school were notable in their lukewarmness to any such 'rational' arguments at all.

#### 4.1.2 Evangelical Anglicans

The attitude of moderate [evangelicals to natural theology was cautiously positive. Charles Simeon noted St Paul's comments in Romans chapter 1 that the Gentiles are 'without excuse'<sup>8</sup>. From the creation they could have traced things up to a 'First Cause'. Simeon asserts that a Supreme Being is generally recognised, but feels that the Gentiles went little beyond this. He feels that natural religion has value only or mainly to those within the light of revelation:

'the variety and beauty of the things existing in this  
terraqueous globe, all so adapted to their respective offices  
and uses, and all subservient to one grand design, the glory of  
their Creator, evince that his wisdom and goodness are equal to  
his power...'<sup>9</sup>

He accepts the watch analogy<sup>10</sup> (he had read Paley), and thinks the world shows signs of design. His words just quoted actually speak of the goodness of God. This might seem to reflect Paley (my point (C) (i) above), but Simeon's is an unsophisticated argument based on the beauty of creation (Simeon, after all, made no claims of a profound philosophy). In any event, to Simeon (as to other [evangelicals) the arguments of natural theology (like the 'evidences' looked at earlier) were of limited value. The real basis of Christianity was faith in God, and Simeon remains ambivalent on how far any such objective arguments may lead to this.

Simeon's fellow leading Evangelical Anglican, J.B. Sumner, seems rather more enthusiastic for natural religion. In 1816 he wrote:

'When, however, the genius of this illustrious age had set up the Protestant faith and the rule of life belonging to it, on an immoveable basis, the attention was naturally directed, in the next place, to those without the pale of Christianity. Accordingly, its agreement on all points with the universal tenets of natural religion, both to inform and to sanction the acquaintances we derive from reason with the Creator and his attributes, and the conformity of the appearance of the universe with the conclusions at which reason arrives; these subjects of perpetual interest have called forth talents worthy of their importance and have received an accession of light from learning, genius and industry, through the successive generations of Stillingfleet, Butler, Warburton and Paley. It is hopeless to look for a vacant spot in a district so fully occupied...'<sup>11</sup>

Sumner's book proceeded to a kind of 'evidences' theme concerning the superiority of the Hebrew records over those of other nations.

This sets the scene for moderate Evangelical Anglican views on natural theology for the first three decades of the nineteenth century. The Christian Observer maintained this approach in its reviews of respective Bridgewater Treatises. Natural theology had some (even though limited) value - as long as kept in its proper place. But, as the editorial commented on Brougham's Discourse: 'A work on natural theology may be essentially either a Christian book or a Deistical one - a useful auxiliary or an insidious rival.'<sup>12</sup>

#### 4.1.3 Evangelical Dissenters

Amongst Evangelical Dissenters, the leading Baptist of the very early nineteenth century was certainly Robert Hall. He was, in fact,

a resident minister in Cambridge from 1791-1804, and then moved to Leicester from 1807 from whence he occasionally revisited Cambridge. He was basically a non-calvinist Evangelical - though ironically not on good terms with Simeon after one of the latter's rare unguarded comments on a religio-political issue.<sup>13</sup> His eloquence was generally regarded as unparalleled, and his reputation stood high amongst both Dissenters and Anglican Evangelicals.<sup>14</sup>

In theology, one can see why his moderate Evangelicalism struck a chord with Sedgwick. He defended the Anglican Evangelicals against Locke's charges of 'enthusiasm'<sup>15</sup>, and thought Laud a bigot and the High Church a 'thinly disguised popery'.<sup>16</sup>

On Biblical inspiration he warns against too great deviation from Scripture words, for the apostle Paul says that he used words taught him by the Holy Spirit:

'We do not, indeed, contend that, in the choice of every particular word or phrase, he was immediately inspired, but we think it reasonable to believe, that the unction which was on his heart, and the perfect illumination which he possessed, led him to employ such terms in the statement of the mysteries of christianity, as were better adapted than [sic] any other to convey their real import ...'<sup>17</sup>

Of Paley's Evidences he says that they are:

'probably, without exception, the most clear and satisfactory statement of the historical proofs of the christian religion ever exhibited in any age or country.'<sup>18</sup>

He himself repeats Paley's arguments about the watch, the structure of the eye as evidence for design, etc.<sup>19</sup> He argues that the suggestion that there was never a beginning, but an eternal succession of mortal creatures, is absurd. There must be some eternal Being. In all this he is quite Paleyan.

On morality, however, he attacks the Paleyan ideas of expediency..

He refers to:

'The simplicity of our ancestors, nourished by the sincere milk of the word... was content to let morality remain on the firm basis of the dictates of conscience and the will of God. They considered virtue as something ultimate ... never supposed for a moment there was any thing to which it stood merely in the relation of a means ...'<sup>20</sup>

'How is it ... we have ... undertaken, after the lapse of six thousand years, to manufacture a morality of our own, to decide by a cold calculation of interest, by a ledger book of profit and of loss, the preference of truth to falsehood, of piety to blasphemy, and of humanity and justice to treachery and blood?' .... In executing this plan, the jurisdiction of conscience is abolished...'

'by a strange inversion, the indirect influence of christianity, in promoting the temporal good of mankind, is mistaken for its principal end...'<sup>21</sup>

Hall's attacks are witty, eloquent, and from a Christian point of view devastating. He mentions Hume as the one who first brought the principle 'into public notice'; but adds: 'It has since been rendered popular by a succession of eminent writers; by one especially, (I doubt not with intentions very foreign to those of Mr. Hume), whose great service to religion in other respects, together with my high reverence for his talents, prevent me from naming.'<sup>22</sup> This was obviously Paley.

The Pauline use of the word 'conscience' (syndeisis) made it natural enough for Evangelicals to reject Paley on this point, and Hall was not the first. As early as 1789 Gisbourne, in his Principles of Moral Philosophy, had attacked 'expediency' as a basis



for morality, and William Wilberforce, for one, agreed with him. But Hall attacked it the most eloquently, and it was his influence, as we shall see, which affected Sedgwick.

We have seen that leading moderate Evangelical Anglicans tended to be sympathetic to natural theology (whilst emphasizing its limitations) but not sophisticated in its application. The Scottish Evangelicals were not only sympathetic but had amongst their leaders figures like Chalmers, Brewster and Miller, whose interest in science and in natural theology was deep rooted and profound. Chalmers, the most important of them, showed an interest soon after his Evangelical conversion, writing in 1817:

'It is truly a most Christian exercise to extract a sentiment of piety from the works and appearances of nature. It has the authority of the Sacred Writers on its side, and even our Saviour himself gives it the weight and authority of his example: 'Behold the lilies...' He expatiates on the beauty of a simple flower, and draws from it the delightful argument of confidence in God...'<sup>25</sup>

How far these Discourses are truly 'natural theology' is doubtful. David Cairns has suggested that they are really more a tracing out of physical illustrations of prior theological beliefs.<sup>26</sup> Yet they do reflect his manifest interest in natural theology, which blossomed in later works.

Without going into Crosbie Smith's distinction between natural history and natural philosophy,<sup>27</sup> we may note his conclusion that 'for Chalmers, God's natural attributes of omniscience, power, omnipresence, unity, eternity and self-existence were known through both natural theology and revelation.'<sup>28</sup> But some further points need also to be made.

First, Chalmers was sceptical of the possibility of discerning the morality of God through physico-theology. In the 1817

Discourses he wrote:

'Without the testimony of an authentic messenger from heaven, I can know nothing of heaven's counsels. I never heard of a moral telescope that can bring to my observation the doings or the deliberations which are taking place in the sanctuary of the eternal.'<sup>29</sup>

He rejected Paley's vision of happy shrimps, together with its associated utilitarian ethic: the world showed many natural disasters, 'wasteful volcanoes' and 'sweeping hurricanes and floods'.<sup>30</sup> Thus 'When the good and the ill of life are looked to in themselves, they seem wholly in-capable of being turned to any theological conclusion which can at all be depended on.' Yet Chalmers did believe that natural theology (as embracing more than physico-theology) could reveal something of God's moral nature. Professor Rice has emphasized Chalmers' belief that the most powerful and convincing argument in nature for the existence of God is in the universality and sovereignty of conscience. He sees in this Butler's influence, noting Chalmers' words that 'It was Butler's Analogy which made me a Christian.'<sup>31</sup> His Bridgewater Treatise was written from a viewpoint that an appropriate study of man's moral nature, even without revelation, reveals much of ethics. Our God-given consciences are such that we automatically experience pleasure when acting virtuously and pain when acting wrongly; a society, moreover, whose members act virtuously, will prosper.<sup>32</sup> Thus he saw arguments for the wisdom and goodness of God as manifested in the creation - as the Bridgewater series indicated by its title. Butler is acknowledged as 'the first who made [conscience] the subject of a full and reflex cognisance'<sup>33</sup>, and Chalmers argues very

strongly that the God-given 'faculty' of conscience is where 'Nature offers to us far her strongest argument for the moral character of God.'<sup>34</sup> On moral philosophy, then, Chalmers (like, as we shall see, Whewell and Sedgwick) accepted Butler against Paley, with little or no discernable influence from Coleridge.

On Natural Theology in general, however, Chalmers placed limitations. First, it should never be used (as Chalmers accused Samuel Clarke and his followers of doing) to pre-determine or pre-judge God's character as given in revelation.<sup>35</sup> Objective reason might test the credentials of a divine messenger, but not judge of the content of his message. Chalmers links this to 'Lord Bacon's philosophy', adding that science flourishes because 'her votaries have learned to abandon their own creative speculations, and to submit to evidence'<sup>36</sup>. This, again, parallels Sedgwick's attitude to such issues, though (as we shall see) Sedgwick was perhaps more consistent in his application of them in areas like the 'Plurality of Worlds' debate. But the true empiricist does not prejudge what 'must' be true either in science or theology.

The second limitation concerns how far natural theology can lead. In this context it has two basic functions. First, it can function (as in Butler) by removing the objections of Infidels or Deists. Secondly, and allied to this, it can show a man clearly the human predicament and problem.<sup>37</sup> So Chalmers asserts:

'In vain do we listen for one authentic word of comfort from any of its oracles ... It can state the difficulty - having just as much knowledge as to enunciate the problem .. Natural theology may see as much as shall draw forth the anxious interrogation, 'What shall I do to be saved?'

The answer to this comes from a higher theology.<sup>38</sup>

Chalmers' whole approach is in the same milieu as Sedgwick's, and it is no surprise to find him expressing appreciation of Whewell's Bridgewater<sup>39</sup> and of Sedgwick's Discourse<sup>40</sup>. A comparison of Chalmers with Brewster and/or Miller would be interesting, but beyond my present scope, though I shall later make some comments on a specific issue.

#### 4.1.4 The Coleridgeans

I have already quoted the words of Coleridge: 'I more than fear the prevailing taste for books of natural theology, physico-theology, demonstrations of God from Nature and the like. Evidences of Christianity! I am weary of the word.'<sup>41</sup> In chapter 3.3 we contrasted attitudes of Evangelicals and Coleridge to Christian evidences in general. Here his words are repeated to emphasize that he specifically applied his criticisms to the areas of natural theology. In contrast to the cautiously favourable attitude of Evangelicals, Coleridge saw natural theology as a positive distraction. This is important to remember when we consider recent claims that the natural theology within the BAAS was allied to a supposedly 'Broad Church' theology. We may also note that Coleridge's emphasis is not particularly Kantian. Kant had attacked the ontological proof as invalid, and had gone on:

'Thus we see that the physico-theological proof, baffled in its own undertaking, takes suddenly refuge in the cosmological proof, and this is only the ontological proof in disguise.'<sup>42</sup>

But Kant himself went on to argue God from necessity arising out of the logical 'practical reasoning' based on the self-evident categorical imperative - a line different from

Coleridgean idealism. Coleridge shares Kant's rejection of physico-theology, but on different grounds.

Coleridge also rejected Paley's utilitarian ethics, but again not along particularly Kantian lines. LeMahieu summarises Coleridge's view:

'(Paley's) ethics and politics were an open declaration of moral bankruptcy. Narrow, superficial, and external, the doctrine of expediency was heathen rather than Christian ... his passionate complaints against Paley bore a remarkable resemblance to the earlier denunciations of the Evangelicals. Both substituted an ethic of inward conscience and spiritual obligation for the abstract, rational, and moral system of Paley.'<sup>43</sup>

#### 4.1.5 Newman and Whately

Newman, in 1830, confusingly acknowledges debts both to Coleridge and Butler in a sermon on natural and revealed religion.<sup>44</sup> Within 'natural religion' Newman (like Chalmers et al) emphasizes the importance of the evidence of indwelling conscience. In this way his 'natural religion' differed from Paley's. But his conclusion is:

'Natural Religion teaches, it is true, the infinite power and majesty, the wisdom and goodness, the presence, the moral governance, and in one sense, the unity of the Deity; but it gives little or no information respecting what may be called his Personality.'<sup>45</sup>

Later (1846) Anglo Catholic and early member of the Oxford Movement W. Palmer produced a book called: The Doctrine of Development and Conscience considered in relation to the Evidences

of Christianity. Baden Powell rather gleefully reviewed this, expressing his satisfaction that Palmer had realised the only alternative to being rational was to be mystical, but declining Palmer's particular conclusions.<sup>46</sup>

Newman himself, in maturity, wrote (1859):

'Ward thinks I hold that moral obligation is, because there is a God. But I hold just the reverse, viz. there is a God, because there is a moral obligation. I have a certain feeling on my mind which I call conscience.

When I analyse this, I feel it involves the idea of a Father and a Judge, - of one who sees my heart, etc.'<sup>47</sup>

It is interesting, incidentally, that in the same year (1859) Whately, who accepted most of Paley, made it clear in his new edition of Paley's Principles that he accepted the Butlerian concept of conscience rather than Paley's moral philosophy. It really does seem to have been a widespread Christian reaction to reject Paley's principle of expediency in ethics.

What we have seen, therefore, is that virtually all schools of nineteenth century Christian thought rejected Paley's ethical system. The Coleridgeans went further in also deprecating his natural theology.

#### 4.1 Notes

1. Paley, Natural Theology, pp. 2-4.
2. Ibid., p. 295.
3. See Yule, PhD Thesis, The Impact of British Religious Thought in the Second Quarter of the Nineteenth Century for comment on both. See e.g. Gladstone's Butler: Subsidiary Studies (1896) p. 109 on Butler's orthodoxy, and D.L. LeMahieu, The Mind of William Paley p. 27 on Paley. Both men's Sermons are also of particular interest.
4. Sermons, cited in Austin Duncan-Jones, Butler's Moral Philosophy.
5. Paley, Principles of Moral and Political Philosophy.
6. Bentham, having discovered Priestley's phrase 'greatest happiness of the greatest number', published his book in 1789. Bentham, of course, refers to a personified 'Nature' rather than to God.
7. Paley (ref. 5), was published in 1785.
8. E.g. Works, 15, p. 17 f.
9. Works, 6, p. 126.
10. Works, 13, p. 226; he also extended the scope to speak in a different context defending his idea of truth in two extremes (see Hennell and Pollard (Eds.), Charles Simeon, p.33).
11. Sumner, A Treatise on the Records of Creation, 1, p. 97.
12. Christian Observer, 1835, 35, p. 697. Other periodicals were similarly aware of the dual possibilities. The British Critic, for example, reviewing Chalmers' Bridgewater Treatise (October 1833, 15, 240-282) defends, with Chalmers, the right use of natural theology in spite of the danger that 'the majesty and supremacy of Revelation should be compromised by all this bustling indagation throughout the regions of Natural Theology.' Noting the infection

of Rationalist ideas and those around us who are 'sinking into the stagnant "oblivious pool" of unbelief', they ask: 'Is natural theology, then, forbidden to stretch out a hand for their deliverance, lest, peradventure, she should land them on the barren shores of Deism...?' The British Critic thinks not.

13. Hopkins refers to this controversy (Charles Simeon, p. 192), though gives a version perhaps rather coloured by Simeon's viewpoint. The letter Hall wrote is printed in The Works of Robert Hall - with a brief memoir of his life, Ed. O.G. Gregory, p.352.
14. The Anglican Christian Observer printed a series of sermons from Hall, and in 1833 reviewed Gregory's book (ref.13). It speaks of his 'extraordinary talents and eloquence', and says that 'whoever went to Cambridge, whether Whig, Tory or Democrat, must as a matter of course' go to hear him (p. 96). The British Critic likewise eulogised him in April 1833 (pp. 292-338).
15. Gregory (ref.13), p. 324, etc.
16. Ibid., pp. 297 and 313.
17. Ibid., p. 285.
18. Ibid., p. 55n.
19. Ibid., p. 59.
20. Ibid., p. 152.
21. Ibid., p. 154.
22. Ibid., p. 158n.
23. See above, chapter 4.1, note 35.
24. The Life of William Wilberforce, 2, p. 235.
25. Chalmers, A Series of Discourses on the Christian Revelation



Viewed in Connection with Modern Astronomy.

26. 'Thomas Chalmers' Astronomical Discourses', Scottish Journal of Theology, 1956, 2, p. 410-421.
27. 'From Design to Dissolution', BJHS, 1979, 12, p. 59-70.
28. Ibid., p. 61.
29. Ibid., p. 90.
30. On the Defects of Natural Theology, Works, 2, pp. 358-420.
31. 'The Thought of Thomas Chalmers', Scottish Journal of Theology, 1971, 24, p. 35.
32. E.g. in his Bridgewater Treatise, 1, p. 188, also p. 178, etc.
33. Ibid., 1, p. 68.
34. Ibid., 1, p. 72.
35. The Evidence and Authority of the Christian Revelation, pp. 274-5.
36. Ibid., p. 276.
37. For their two points see Rice (ref. 31), p. 37-39.
38. Chalmers (ref. 30), 2, p. 417.
39. Works, 1, 193n.
40. Letter to Whewell, Trinity Collection Add Ms a 202.25.
41. See above p. 180 f; quotations are also given to show similarities in the approaches of Hare and Maurice.
42. Critique of Pure Reason, Div 2; 3.6.
43. LeMahieu, The Mind of William Paley, p. 157; other books on Coleridge (e.g. Barth, Coleridge and Christian Doctrine, p. 195; Boulger, Coleridge as a Religious Thinker, p. 33; Garland chs. 3 and 4) also comment on this. The quotation has been given fully to

illustrate the irony that, whilst specifically recognising the similarity of attitude to Paley of Coleridge and the Evangelicals before him, LeMahieu (as we shall see in part 4.2.1) appears to miss the Evangelical influence on Sedgwick on this issue, and speaks of the inspiration of Coleridge.

44. The Influence of Natural and Revealed Religion Respectively  
p. 23n; p. 31.

45. Ibid., p. 22.

46. Edinburgh Review, Oct. 1847, p. 397-418.

47. Unpublished paper given in The Argument From Conscience to the Existence of God According to J.H. Newman by Boekraad and Tristram. From a rather less 'High' position, the British Critic, reviewing Abercrombie's The Philosophy of the Moral Feelings (published 1833, reviewed with Whewell's Bridgewater July 1833, vol. 2, p. 72-113) states: 'It was the achievement of Butler to assert the supremacy of conscience and to expose the calumnious sophistry which resolves all our best dispositions and actions into mere modes and manifestations of selfishness.' (p. 85). It then continues to attack Paley's 'Low and defective representation of moral goodness.'

## 4.2 Sedgwick and Natural Theology

### 4.2.1 Introduction

As we have seen, a number of modern writers assume that Sedgwick (amongst others such as Whewell) shared some kind of 'Broad Church' membership with a group amongst whom Hare and Maurice were leaders and who were much influenced by Coleridge. Cannon actually goes as far as to portray Sedgwick as part of a 'scientific node' which, with a 'theological node', comprised one set of people he calls the 'Cambridge Network'. Coleridge's supposed influence has also been specifically connected with Whewell and Sedgwick's rejection of Paley's ethics. LeMahieu, who, as we saw, noted Coleridge's similarity to the Evangelicals on Paleyan ethics, was nevertheless specific on the inspiration of Sedgwick's views on the issue:

'What finally broke the hegemony of the Principles [i.e. Paley's on morality] was its repudiation by a small but enormously powerful group of Cambridge thinkers who, inspired by the rising star of Coleridge, despised utilitarianism and wished its influence swept from the university. Their antipathy found one of its most powerful expressions in Adam Sedgwick's Discourse on the Studies of the University...<sup>1</sup>

LeMahieu's book is interesting on Coleridge, but his main cited sources for Sedgwick appear unreliable. From Winstanley he has derived the contentious view that Sedgwick was 'Temperamental and often outrageously unfair'<sup>2</sup>, from Gillispie he has some not very accurate comments about Sedgwick's controversy with Lyell, and the comments on ethics contain a footnote to Cannon's 1964 article on 'The Cambridge Network'.

Martha McMackin Garland, in Cambridge Before Darwin, though

more careful in the way she phrases it, still seems (in page 88, note 107) to imply that 'Coleridge's influence was important' in the views of the Cambridge scientists who contributed to 'the University's rejection of Paleyan thought in favour of a more organic social view.'

Thackray and Morrell are much less careful:

'Trinity college was a seminary of the Broad Church or liberal Anglican Movement. J.C. Hare and Connop Thirlwall, who were Trinity tutors, helped to articulate that blend of Kantian idealism, toleration, and Coleridgean conservatism which came to characterize the Movement, and which appeared as much in the scientifico-moral tracts of Whewell as in the Movements more directly theological and political writings. The liberal Anglican theology of the 'Cambridge Network' rapidly became the unofficial credo of the BAAS, with important theological and practical results.'<sup>3</sup>

The assertion of this thesis is that the Coleridge connection, if not the whole concept of the 'Cambridge Network', is largely myth. Our particular concern here is with Sedgwick, and the present chapter focuses on him. But he has been linked particularly to Whewell, especially in their common rejection of Paleyan ethics, and so some comment on Whewell's position on these issues will also be helpful. It must be said that Whewell appears less Evangelical in tone than Sedgwick, and certainly stood closer to Hare in personal terms. Yet even his natural theology was not Coleridgean in tone, and (as we shall see) his moral philosophy was consciously related to Butler rather than Coleridge. As for Sedgwick, far from sharing any supposed 'liberal Anglican theology' in the BAAS, his theology was nearer to a 'naive brand of Paley and Bible' which (as Garland says) the Coleridgeans deprecated. His moral philosophy and rejection of Paleyanism (again as we shall see) was consciously based on the

Evangelical critique of Robert Hall. Both in moral philosophy and in natural theology Sedgwick stood much closer to the Evangelical traditions than to Coleridge.

The evidence for these assertions will be presented presently, but it will be useful to note at this point the way in which the dominant idea of supposed 'liberal Anglicanism' within Thackray and Morrell leads to a total obscurity of the moderate Evangelical position (as represented by the Christian Observer and the Scottish group around Chalmers). Frederick Nolan is taken<sup>4</sup> as typical of 'Evangelical Christians given to a literal interpretation of Genesis'. The 'enchantment' of Chalmers with the 1833 BAAS meeting, the fact that he was 'particularly impressed by Sedgwick' and became intimate with Sedgwick and Whewell, the fact that he as the leading Scottish Evangelical met there the leading English Evangelical Simeon (though not for the first time as they seem to imply) are all mentioned<sup>5</sup>. Yet on p. 237 we read:

'In 1833 at Cambridge, Sedgwick as President pressed the case for natural theology, being conveniently joined by Thomas Chalmers, the evangelical Scottish Presbyterian. No fewer than five authors of Bridgewater Treatises (Whewell, Buckland, Chalmers, Prout and Kirby) were present at the 1833 Meeting, thus confirming the close alliance between the British Association and the liberal natural theologians.'

The irony of putting the leading Scottish Evangelical theologian of his period in with the 'liberal natural theologians' is immense. But the phraseology makes it sound as though Chalmers' presence was almost fortuitous - a lucky accident for

the theological liberals (under the spell of Coleridge) who led the BAAS. This is simply not so. The traditions on which they drew, and especially in the case of Sedgwick, were primarily Paleyan and Evangelical, and the presence of Chalmers was because he stood in a similar tradition.

#### 4.2.2 Sedgwick's Natural Theology

Sedgwick first published his University sermon Discourse on the Studies of the University in 1833 (just before Brougham's 1835 edition of Paley's Natural Theology). In it he divides up study into 'the study of the Laws of nature', 'the study of ancient literature', and 'the study of ourselves' (including ethics, metaphysics, and moral and political philosophy). He is writing, of course, on the assumption that his readers are Cambridge students - who are therefore already committed to the Christian religion. He defends the study of science

'The laws by which God has thought good to govern the universe are surely subjects of lofty contemplation... studies of this kind not merely contain their own intellectual reward, but give the mind a habit of abstraction... and a power of concentration of inestimable value...'<sup>6</sup>

Sedgwick also feels, and cites Job 38 as illustration of the Biblical principle, that to study nature is humbling - for we can contrast our own ignorance with the wisdom and power of God.<sup>7</sup> He continues:

'A study of the Newtonian philosophy ... teaches us to see the finger of God in all things animate and inanimate, and gives us an exalted conception of his attributes, placing before us the clearest proof of their reality- and so prepares, or ought to prepare, the mind for the reception of that higher illumination, which brings the rebellious

faculties into obedience to the divine will.'

In an appendix (C in 1st Edn., F in 5th) Sedgwick considers the role of Natural Religion:

'It has been sometimes objected to natural religion that it misleads men by a dim and deceitful light, on matters clearly put before them in the word of God. The objection is sound, so far as it applies to those who set up natural, in the place of revealed truth: but it is invalid in any other sense, as it strikes at the root of all knowledge that is not religious. If the conclusions of natural religion be true, then must they well deserve our study; and they are of no small moral worth, provided they be kept in their proper place, and in subordination to truths of a higher kind. Were all men honest believers in the word of God, then could the inductions of natural religion add nothing to the strength of their convictions of his being and providence. But to doubting minds, entangled in the mazes of a false philosophy, or lost perhaps in sense, and unused to any severe exercise of thought, the ready inductions of natural religion may bring convictions of the greatest moral worth - at moments, too, when proofs of a different kind would be denied all access to the understanding. Moreover, the habit of contemplating God through the wonders of the created world, and its adaptation to the wants of man, is not only compatible with firm religious belief, but with the highest devotional feeling; as is proved by passages, almost without number, in the sacred poetry of the Bible.<sup>8</sup>

The interesting thing here is that, whilst Sedgwick thought natural theology might prepare someone for Biblical revelation,

he sees a greater role as enhancing the Christian's whole view of nature. Brooke's sentence: 'It almost evaporated into the spirit in which the natural sciences should be pursued'<sup>9</sup> is true - but such a statement could very easily be misunderstood. The spirit in which the sciences should be pursued was more than 'cheerful sobriety', it was an intense 'God-consciousness' which permeated Sedgwick's own study of nature. Brooke's article later comments on Pye Smith's reaction to Lyell's use of the word 'nature' instead of God as active agent: 'O why did not his heart grow warm within him, and bound with joy at the opportunity of doing homage to the God of glorious majesty ?'<sup>10</sup> The answer, surely, is that whilst Lyell believed in a God, his correspondence<sup>11</sup> does not exhibit the kind of personal piety and personal experience of God one finds in Sedgwick - or, e.g., in Kingsley<sup>12</sup> whom Lyell regarded highly. The Evangelicals of all schools, the Coleridgeans, and Oxford school, mostly shared this kind of 'God-consciousness'. The passage, cited from Sedgwick above, reflects this. He does (unlike the Coleridgeans) think that Paley's argument has logical validity - but more than that he hopes that the students will catch Paley's own God-consciousness as they read it. It is an experience of God in his creation to which, as Christians, he urges them.

In fact, Sedgwick included within the term 'natural religion' three distinguishable elements. These are (i) religion according to Reason (ii) the innate capacity of the human mind to receive and conceptualise religious terms (iii) the common pre-christian religions. Thus to begin with:



'No wonder that the truths of natural religion should have oftener been pressed by Christian than by heathen writers. The Christian makes them not the foundation of his faith. He seeks them not because he doubts but because he believes: for they are the external means of communion with a being whom he is taught to love, and in whose immediate presence he hopes hereinafter to dwell.'<sup>13</sup>

In the next paragraph Sedgwick says: 'let us suppose some one to ask how we came by the knowledge of God.' There are two alternatives: by his works and by revelation. If someone tries to insist that God speaks not through his works but only through revelation:

'We may then ... ask, what knowledge could mere sounds like these convey through the ears to the heart, were there not placed already within it some knowledge of the being of a God; or at least were there not in the soul some natural and inborn power of rising to the apprehension of a general religious truth when presented to it in the form of a mere abstraction.'

But this kind of innate capacity is to be distinguished from a logical reasoning out of the basis of religion. Thus Sedgwick also says:

'The logical order and the historical order of our belief are two things widely different; and this is true in every department of our knowledge: we believe first on authority and we examine afterwards ... what we call natural religion did not, I think, in any distinct and logical form, historically precede revealed religion, but followed it.'<sup>14</sup>

He continues to affirm that our religion will stand up to our

'most searching examination of reason', but that we first believed on authority and later looked for the logical grounds of belief. When we look for a starting point to reason from, we find it in: 'the truths of natural religion (or, which comes to the same end, the elements of a religious nature within ourselves).'<sup>15</sup>

This kind of approach is surely of the same ilk as that of Chalmers and Miller? Experience of created things makes the concept of a Creator meaningful - yet a consciously explicit 'natural theology' is not a necessary pre-requisite nor a substitute for faith.

#### 4.2.3 Natural Theology and Moral Philosophy

We saw in chapter 4.1 how Paley rejected 'moral sense' as a basis for ethics, and used instead a principle of 'utility' or 'expediency' as it came to be called. It was Paley's system which, very soon<sup>16</sup>, was adopted in Cambridge. In the second quarter of the nineteenth century, however, both Sedgwick and Whewell were in the forefront of a reaction in Cambridge against this. Some recent historians of science, (as quoted at the start of this chapter), have emphasized a supposed influence on the Sedgwick circle of the 'rising star of Coleridge'. Is there, then, any evidence for or against such an influence? The remainder of the present chapter will consider this question.

As far as concerns Sedgwick the evidence seems very clear that the main influence on this issue was a man who was far from fitting into any theory of a supposedly liberal Anglican 'Cambridge Network': none other than the Evangelical Dissenter Robert Hall. We have his own testimony on this in a footnote to a section dealing with the objection that natural theology is no use because man is totally

corrupted (a view which an extreme Calvinist might take). Sedgwick says that the works of men like Chalmers and Robert Hall showed 'how possible it is for man to feel a deep conviction of the natural depravity of his heart, at the time that he has sublime and philosophic views of those moral and intellectual capacities he derives from God.'<sup>17</sup> In a footnote to Hall he adds:

'In mentioning the name of Hall, I may, I hope, be permitted to state that on reading (now many years since) some of his wonderful discourses, I first learned to doubt the truth of that system which regards utility as the test of moral right.

At a time when the doctrine generally prevailed in England, he set himself against it, with a power of moral reasoning - with a subtlety and fervid eloquence, which placed his works at once amongst the highest productions of the human mind.'

He then goes on to eulogise Hall's Modern Infidelity Considered (1800) and Sentiments Proper to the Present Crisis (1803). Sedgwick, in fact, wrote to the Evangelical Moule in 1868 of his recollections of Hall. The latter had ceased to live in Cambridge before Sedgwick's year, but Sedgwick went to hear him whenever he came to preach and says he had 'a power of eloquence such as I have never felt or witnessed in the speaking of any other man.'<sup>18</sup> We cannot, of course, know exactly when Sedgwick first read Hall, though he says 'many years since' in 1833. But Sedgwick's turn of phrase clearly resembles Hall's. Hall wrote of Paley's system that morality was:

'... by a cold calculation of interest, by a ledger book of profit and of loss...'<sup>19</sup>

Sedgwick likewise complains that under Paley's system:

'Virtue becomes a question of calculation - a matter of profit and loss; and if a man gain heaven at all on such a system, it must be by arithmetical details... the balance of the moral ledger...'<sup>20</sup>

On the same page Sedgwick asserts his belief that Paley would not have been capable in his later years of such sentiments. He then denies that moral sentiments come either by tracing out the results of wrongdoing, or by teaching - for the moral sense is often strongest in very early life. Neither logical ethics, nor even revealed ethics, could be effectual without an 'inherent moral capacity.'

Paley, in his Lockean empiricism, had denied an innate moral knowledge. Sedgwick responds: 'No one now speaks of an innate knowledge of morality; an innate moral sense or faculty, defining and determining the quality of our moral judgements, is all for which we contend...'<sup>21</sup>

Sedgwick confesses his incomprehension at how any Christian can resolve all actions into the effects of mere selfish passion:

'If we suppress the authority of conscience, reject the moral feelings ... the sinner is no longer abhorred as a rebel against his better nature - as one who profoundly mutilates the image of God; he acts only on the principles of other men, but he blunders in calculating the chances of his personal advantage...'<sup>22</sup>

Sedgwick also cites Butler in his Discourse (and elsewhere),<sup>23</sup> and stands in the same tradition as Butler, Hall, Chalmers et al. It would not be useful, for present purposes, to go into more detail.

Two comments on Mill's critique<sup>24</sup> of this part of the Discourse may be relevant (Mill used the 3rd Edition which is virtually unchanged from the 1st). Firstly, Mill begins by admitting that he believes the main object of universities is to 'keep alive philosophy'. He tries to read Sedgwick's Discourse as though it was intended as an original treatise to advance philosophy - and finds it untechnical and too simple (citing only Locke and Paley amongst the utilitarians).<sup>25</sup> But the Discourse as originally conceived (before the 5th Edition included massive preface and appendices) was a pastoral one.

Sedgwick was concerned not for the philosophical 'shooting stars' which concern Mill, but for raising young Christian men of character.<sup>26</sup> He dwells on Locke and Paley because they are the set reading for students - and if early Cambridge students are at all like their modern counterparts then probably few went beyond the set reading. Secondly, it would seem that Mill's whole inductive, utilitarian philosophy was unlikely to find conducive any formulation which was at once Kantian in its reaction to Locke-Hume, and intensely Christian. Thus, for example, Mill laughs at Sedgwick for expecting a work (Locke's) on human understanding to include imaginative powers.<sup>27</sup> 'What place, what concern, could it have had there?' he asks. None, of course, if the process of obtaining understanding is purely passive - but that is exactly the point at issue. Sedgwick is arguing that:

'It is certain that the glories of the external world are so fitted to our imaginative powers as to give them a perception of the Godhead, and a glimpse of his attributes.'<sup>28</sup>

Viewed thus, what Sedgwick calls 'imaginative powers' are a source of understanding - not merely a creative force 'as a course of enjoyment, and a means of educating the feelings' as Mill seems to suppose. To Mill, Locke's essay was 'the beginning and foundation of the modern analytical psychology'.<sup>29</sup> To those of us who do not share Mill's assumptions, Locke, and even more Hume, are precisely at their weakest when dealing with the meaning of mind and consciousness - and Baconian induction is at its weakest when concepts of 'cause' are based (as Mill tried without consistency to do) on some kind of law of uniformity rather than a neo-Kantian presumption of a mind looking for patterns. This is not, of course, to say that none of Mill's criticisms of Sedgwick were justified - but that they were

of such fundamentally different approaches that misunderstanding was only to be expected.

Before bringing this chapter to a close I would like to add some comments on Whewell. We have seen how Whewell is linked to Sedgwick by modern commentators on the anti-Paley-ethics reaction, and we know also that both were attacked by Mill. Certain things may be said about Whewell:

(a) He was one of the few in Sedgwick's circle who really understood and was familiar with Kant.

(b) His philosophy of science was more sophisticated than both that of Sedgwick and that of Mill (see below in chapter 7.5).

(c) His writing was less obviously Evangelical in tone than Sedgwick's, and he also stood closer in personal terms to Hare.

If anyone in Sedgwick's immediate circle might have been expected to exemplify this supposed 'Coleridgean' link it is surely Whewell.

What is the evidence? First, Whewell's Bridgewater Treatise in 1833 is very much in the Paley/Hall kind of tradition. It shows little affinity with Coleridge's approach.<sup>30</sup> On the specifically moral issues, Whewell wrote the famous Elements of Morality (1845) and Lectures in the History of Moral Philosophy in England (1852). But he had already set out his views in the earlier Sermons (1837). After criticising Paley's moral philosophy as having 'had a very large share in producing the confusion and vacillation of thought respecting the grounds of morals, which is at present generally prevalent in England', Whewell continues:

'The writer whom I have adduced as the principal representative of a better system than Paley's is Bishop Butler.'<sup>31</sup>

Whewell admits that Butler did not write systematically on the subject, nor did he do what Paley's work professes to do: 'point out the rational grounds of the good institutions which prevail in organised

societies... and thus invest those institutions with the sanction of morality as well as law.'<sup>32</sup> Whewell says that no such systematic treatment then exists, but he trusts that his sermons in illustrating Butler's principles 'might be of great use in the absence of a more complete system.'

Whewell, like many contemporaries, fastened in his exegesis on the Pauline concept of 'conscience'.<sup>33</sup> The created world, he says, may show the Creator to be powerful, but at this level natural theology tells us nothing of the moral nature of God. We discover this through the inner functioning of a God-given conscience. The conscience is central throughout Whewell's sermons, and his treatment resembles both Butler and Chalmers. Not, of course that Whewell is advocating obedience to conscience and self effort as the essence of Christianity:

'it is not the approval of our conscience, but the mercy of our Redeemer which is to be our final stay and support'

But Whewell is treating of morality in a 'natural theology' context - as he understands St Paul to be doing in Romans 2.15 etc. - of conscience as an indication to a person (before any revelation) of the existence of a moral God.

Whewell makes no reference to Coleridge in his sermons, and the language of his objections to Paley's moral philosophy is quite different from that of Coleridge. Even, then, where we might have expected a 'Cambridge Network' Coleridge connection to be the most obvious, we find instead that (although he recognises common cause with his friend Hare against utilitarian ethics) Whewell's system is built on a Pauline term elaborated by Butler. If there is indeed any further 'mediation' of the concepts, it would be more plausible to look for it in the areas where links may so clearly be established for Sedgwick - amongst the leaders of evangelical Dissent.

4.2 Notes

1. LeMahieu, The Mind of William Paley, p. 159; we already noted (p. 5) Cannon's implication that it was admiration for Wordsworth and Coleridge which formed the basis of Sedgwick's attacks on Paley's ethics.
2. Ibid., p. 159.
3. Thackray and Morrell, Gentlemen of Science, p. 226.
4. Ibid., p. 234.
5. Ibid., p. 173.
6. 1st Edition Discourse, p. 10.
7. Ibid., p. 11.
8. Sedgwick is here recognising the dangers in using natural theology as an alternative to revelation, rather than as an encouragement to accept the God of revelation. His words are very similar both to Chalmers and to the Evangelical Christian Observer, as well as to the British Critic of the same period (early 1830's).
9. Brooke: 'The Natural Theology of the Geologists...', in Images of the Earth (Eds. Jordanova and Porter), p. 42.
10. Ibid., p. 49.
11. This is shown throughout Lyell's Life, Letters and Journals...
12. See F.E. Kingsley, Charles Kingsley: His Letters and Memories of his Life.
13. 1st Edition Discourse, Note D.
14. 5th Edition Discourse, ccli.
15. 5th Edition Discourse, cclii.
16. Garland, Cambridge Before Darwin, ch. 4 describes this. The



moral philosophy of Paley was actually introduced into the curriculum by Sedgwick's old Tutor Thomas Jones. Sedgwick's regard for Jones (Clark & Hughes, 1, p. 91), and his first experience of being charmed by the book, did not prevent his later total rejection of its concepts under the influence of Robert Hall.

17. 1st Edition Discourse, Note D, p. 54.
18. Clark & Hughes, 1, p. 80. Hall was, of course, a highly educated man who had studied at Aberdeen University. He was familiar with Butler by the age of nine (see Gregory in Works of Robert Hall... p. 4). In a sermon he might, for example, cite Bacon on reason and revelation (as in a Fast-Day Sermon, October 19th 1803), and the chord struck in Sedgwick is not surprising.
19. Works of Robert Hall, p. 154, and see chapter 4.2 above.
20. 1st Edition Discourse, p. 57.
21. Ibid., p. 50.
22. Ibid., pp. 62-65.
23. Ibid., p. 97n.
24. Mill's Critique appeared in the London Review, 1835.
25. Garland (Cambridge Before Darwin, p. 68) sees the Cambridge Dons' reactions against Paley as part of the 'Victorian Spirit' with its 'earnest and serious' approach. She adds: 'It is thus not surprising that Sedgwick and Whewell contributed little that was original to the progress of moral thought in England...' Garland, of course, emphasizes the 'Broad Church' links and totally ignores Hall and the Evangelical ones. But she seems here to miss the point that Sedgwick did not suppose that he was making any contribution to the 'progress of moral thought', and that not everyone shares Garland's evidently liberal optimism that

moral thought is progressing or, in deed, can progress.

26. See e.g. 1st Edition Discourse, p.7.

27. Mill, Essays, p. 118.

28. 1st Edition Discourse, p. 18; (3rd Edition identical).

29. Mill (ref 27), p. 120

30. It was also well received in Christian Observer circles, e.g.

being Simeon's favourite work (after the Bible) during recovery.

from an illness (Carus, Memoirs of the Rev Charles Simeon, p. 722);

the British Critic (1833, 14, 92-113) eulogised both it and Whewell.

31. Sermons (Preface), p. vii; Whewell's contemporaries recognised

his sources; thus e.g. Pryme (sometime Professor of Political

Economy) says that Whewell rejected Paleyan ethics and: 'built

anew a system founded on the existence of a moral sense, as

supposed by Bishop Butler, author of the Analogy.' (Bayne,

Autobiographical Recollections of George Pryme, p. 332).

Whether he was also influenced by Hall is hard to say. He wrote in 1823: 'Robert Hall, the great Baptist minister of Leicester, has lately been here, and I heard several of his sermons. I certainly consider him one of the most eloquent and striking preachers I ever heard or hope to hear.' (Life of Dr Whewell, Ed. Douglas, p. 93)

In general, however, Whewell's attitude to Evangelicals was less uniform than Sedgwick's. In the same letter he is critical of a sermon of Simeon's, for whom his sympathy is evidently less than Sedgwick's. He does, however, like Chalmers' work (p.74), and calls, in an 1828 letter, the Evangelical J.B. Sumner (newly appointed Bishop of Chester) a: 'most excellent person. His books are very good.' Whewell was more sympathetic to Evangelicals than is often realised.

32. Whewell, Sermons, p. x.

33. Ibid., p. 19.

### 4.3 Natural Theology and the Plurality of Worlds Debate

#### 4.3.1 The Nature and Significance of the Debate

An area of natural theology which flared into controversy was the question of whether or not other 'worlds' were inhabited by rational creatures. In 1853 Whewell, in an anonymous work argued that they were not, whilst Brewster, in 1854, took the opposite view.<sup>1</sup> Sedgwick published only a brief note on the issue and most of his views on the matter are privately expressed. Nevertheless, the debate is of interest to us, for it is yet another aspect of Sedgwick's metascientific milieu which some modern scholars have seen in terms of the 'Cambridge Network' and its supposedly liberal Anglican theology. John Hedley Brooke, in an article which contains much interesting detail, asks why the two leading protagonists took the sides that they did.<sup>2</sup> He notices their uneasy relationship of suspicion, and also their respective interests in astronomy and geology as potential sources of difference, but he says:

'I have tried to show that the positions which Brewster and Whewell adopted on the question of extra-terrestrial life and, associated with those positions, the respective genre of natural theology which each defended, were dependent on the religious tradition to which they were most closely affiliated.'<sup>3</sup>

Brooke makes similar emphasis in his summary, adding that it suggests a 'fragmented and disordered state of natural theology' at the time.<sup>4</sup> The caveats Brooke places on this generalisation<sup>5</sup> may not be noted if his conclusion is cited elsewhere; and he himself cites it without the caveats in Images of the Earth.<sup>6</sup> He has chosen to emphasize not the idiosyncrasies of Whewell and Brewster, but their respective associations with 'Broad Church' or 'liberal Anglican' and 'evangelical' traditions. This, coupled with Brooke's apparent acceptance of a Cambridge 'Broad Church Network'<sup>7</sup> leaves an impression

that the natural theology of the Network members (presumably including Sedgwick) differed from that of the Evangelicals. The present chapter, therefore, attempts to answer two questions. Firstly, what were the attitudes of various church parties to the plurality of worlds issue, and did the views of Whewell and Brewster respectively reflect those of their religious traditions? Secondly, what were Sedgwick's views on the issue and with whom (if anyone) did he align?

Let us, therefore, first consider Brewster and the Evangelicals, and in particular whether his views were in any sense representative. Brooke's article itself contains a wealth of references to different Evangelical commentators, though his overall perception of the situation appears to differ somewhat from my own. Brooke admits that Brewster's 'proof text' for his doctrine was unconvincing<sup>8</sup>, and that the Christian Observer (still a mouthpiece for the most highly placed Anglican Evangelicals) found Brewster's approach to exegesis similar to that of the Mormons.<sup>9</sup> He is unable to cite any important Evangelical, in England or Scotland, who supported Brewster's extreme position. With a footnote again to the Christian Observer he agrees: '... there was general agreement among other apologists that the safest strategy was to show that it mattered not one jot to Christian doctrine whether there were other worlds or not.'<sup>10</sup>

Brewster's position was not supported by English Evangelicals, but was it supported by the leaders of his own Scottish denomination? Two of the most important of these were Chalmers and Miller. Chalmers, in his 1817 treatment of the issue, had not been arguing that the plurality of worlds was essential to Christian faith. Rather, taking it for granted that life and intelligence existed elsewhere<sup>11</sup>, he was concerned to show that this presented no problems to Christian theology. Chalmers deals with objections based on the uniqueness

of the atonement, and also against arguments that man is beneath God's concern. The latter suggestion, he says, belittles God. 174

Miller took up this same question in 1846:

"Does not the largeness of that field which astronomy lays open to the view of modern science throw a suspicion over the truth of the gospel history? and how shall we reconcile the greatness of that wonderful movement which was made in heaven for the redemption of fallen man, with the comparative meanness and obscurity of our species?"<sup>12</sup>

Miller does not state Chalmers' answer, but asserts that geology has 'completely annihilated' the objections 'with which Dr. Chalmers grappled so vigorously'.<sup>13</sup> Miller argues that geology has shown that for most of earth history there have been no 'accountable creatures' or a 'rational being, born to anticipate a hereafter'. Thus, he argues, there is no reason to suppose that other worlds necessarily have rational and moral beings on them.

It is useful to note several things about this. First, Miller (and, though more tentatively, Chalmers) admits the real force of the infidel argument - he is prepared to admit the reality of the doubt even though later claiming that geology has 'completely annihilated' it. Like Brooke's description of Whewell, Miller is 'admitting doubts in order to assuage them' - and does not share the apparent aversion of his fellow Evangelical Brewster to admitting any doubts at all. Secondly, Miller does not merely argue that geology removes the proof or probability that other worlds contain rational and moral beings - he says this in a context of answering objections that man is not important enough for God to take trouble over him. In other words, man is important as he may be the unique rational and moral agent at present existing. A similar line of argument in Whewell is contrasted by Brooke with: 'an evangelical stress on the depravity of man and the condescension of God'.<sup>14</sup>

A third point is that Miller is not, of course, arguing that other worlds must always remain without rational life. This would not be necessary for his purpose of arguing the importance of man.

Now Whewell, as Brooke makes clear<sup>15</sup>, argued a more extreme position than this. He was generally taken to mean that other worlds would never boast intelligent life - and the analogy from geology simply could not imply this. Surely, however, Brooke's summary is too strong: 'Though he anticipated Whewell's central argument, Miller did not stretch it in the same direction,'<sup>16</sup>. Miller did take it in the same direction- he, like Whewell, was arguing that man was not so insignificant as the Deists claimed. It is as though Miller was saying 'Man is not insignificant, as he may be the only rational being at present in existence', and Whewell added 'and always will be'. This is not a change in direction, it is a matter of degree. In actual fact even this must be qualified, for on page 330 Whewell conceded that there may be other realms with 'subjects and servants of God' as many pious souls have thought - but they must not be placed on the planets or stars.

Whilst the direction of argument of Miller and Whewell seem to be the same, one might agree with a statement that their respective traditions coloured the language they used. Brooke quotes Whewell as saying of man: 'God may think him worthy of his regard and government'. Other language of Whewell's book (e.g. his reference to 'moral trial' of man on earth) is not Evangelical in tone. But this does not constitute a fundamental difference in natural theology.

Miller, then, was closer to Whewell than to Brewster. Yet, as Brooke himself admits, Whewell found little support for his extreme position in any quarter - Evangelical or liberal - and Brooke is unable, in fact, to find any prominent 'liberal Anglican' who even sympathised with it.<sup>17</sup> What was Whewell really about?

This question, it seems, puzzled Whewell's contemporaries, and the various suggestions of modern scholars are not convincing. Brooke suggests Whewell was obliquely attacking Chambers, who saw plurality as an argument for transformation of species.<sup>18</sup> But in assessing this we must ask whether there is force in Chambers' objections to multiple creations: 'Is it conceivable, is it a fitting mode of exercise for creative intelligence that it could be constantly moving from one sphere to another...?'<sup>19</sup> Not only had Chalmers answered this years before, but in this form it is so fatuous a caricature of the views of a progressive creationist belief in an omnipresent God that Sedgwick, for example, could hardly believe that anyone could be so silly. He wails:

'Who but a man whose mind had been cramped by the fetters of a rank materialism would dare to write... such irreverent nonsense ? ... Who is it that anthromorphises his Maker, and thinks him weary whilst journeying from one organic creation to another ?...'<sup>20</sup>

Sedgwick is surely right. To anyone who believes in the power of a God who can (for example) listen to millions of prayers simultaneously to suppose any difficulty in Him operating simultaneously in different worlds is utter folly. Yet Brooke supposes Whewell to have taken this seriously, and to obliquely challenge Chambers' premise.<sup>21</sup> He suggests that Whewell rejected a direct Sedgwickian attack on Chambers' scientific absurdities because its readership was ill informed<sup>22</sup>, and then himself attacked it with a theory which (a) he knew few scientists would accept anyway, and (b) depended on esoteric distinctions in the senses in which the nebular hypothesis could be used.<sup>23</sup> Brooke's suggestion was only very tentative<sup>24</sup>, and it simply is implausible.

Yeo provides an alternative suggestion that Whewell wished to argue from the uniqueness of man's mind to its similarity with God's<sup>25</sup>, though does not make clear the logical connection. Hefferman suggests

Whewell was trying to remove fantasy elements and promote clear thinking<sup>26</sup>, though most readers thought him anything but clear.

Whatever, however, the motives of Whewell and Brewster, one thing should be clear to us. Their attitudes cannot be seen as indicative of deeper underlying rifts between the natural theology of Evangelicals and liberal Anglicans (assuming, for the present, that the latter is adequate as a description of Whewell). Whewell gave an extreme version of Miller,<sup>27</sup> whilst Brewster gave an extreme version of Chalmers. Thus Whewell refers to Miller's earlier work as having 'presented an argument from geology very much of the nature which I have employed'<sup>28</sup> and Miller, whilst critical of both men's views, refers to 'our position' and finishes with a 'very admirable' passage from Whewell:

'Let the difficulty be put in any way the objector pleases. Is it that it is unworthy of the greatness and majesty of God, according to our conception of Him, to bestow such peculiar care on so small a part of his creation? But we know from geology that He had bestowed upon this small part of creation - mankind - this special care. He has made their period, though only a moment in the ages of animal life, the only period of intelligence, morality, religion. If, then, to suppose that He has done this is contrary to our conceptions of His greatness and majesty, it is plain that our concepts are erroneous: they have taken a wrong direction. God has not judged as to what is worthy of Him as we have judged. He has found it worthy of Him to bestow upon man his special care, though he occupies so small a portion of time; and why not, then, although he occupies so small a portion of space?'<sup>29</sup>

This is in the Butlerian tradition of Chalmers and Miller. The strongest argument for God (said Chalmers) was the conscience. It was natural enough with this view (which they shared) to see



morality and religion in man as a mark of special care for man. Man in this sense is unique in earthly history - so why should he not also be unique in space? And if so the special position of man in the scheme of Christianity (with redemption etc) becomes plausible. Surely Miller was not mistaken in quoting this passage as 'admirable' and along the lines of his own thinking? Chalmers, in fact, seems himself sympathetic to Whewell, writing to him of his 'surprise' at Brewster's review, and the 'injustice and severity' of periodicals.<sup>30</sup>

Whilst, then, there was little support for Whewell's extreme position, Evangelicals were at least as sympathetic to him as to Brewster. We have seen this for Brewster's own close associates, and we earlier noted the Christian Observer's criticism of Brewster's article which it found 'surprisingly full of errors' and took a middle path.<sup>31</sup> Thus, although the Brooke article has provided us with a wealth of useful detailed material, the overall framework within which these detailed facts are perceived is not convincing.

#### 4.3.2 Sedgwick's Views on the Debate

Sedgwick, as Brooke asserts, was rather more conservative theologically, and more biblically orientated than Whewell.<sup>32</sup> In any event, he did not share Whewell's views on the plurality of worlds. In a letter to Herschel<sup>33</sup> he asserts that, having read Whewell's book: 'I was much amused by it, but not convinced.' A copy of the work is extant in the geological museum of Cambridge with marginal notes in Sedgwick's handwriting. Sedgwick is highly critical of Whewell's arguments, though he himself does not come down on one side or the other. On the religious side: 'No one says that a belief in a plurality of inhabited worlds is an article of faith'.<sup>34</sup> On the scientific side: 'From the nature of its case - positive evidence (either + or -) is out of question'.<sup>35</sup> Thus it is a

matter of 'speculative probabilities'.<sup>36</sup> Whewell insists that 'We need no speculations about the inhabitants of other worlds to make this world our place of moral probation.' Sedgwick asks 'why on that account say that other worlds have no beings under an analogous probation'?<sup>37</sup>

When Whewell insists that 'one theatre of moral action, one arena of moral contests for the highest prizes is a sufficient centre for innumerable hosts of stars and planets',<sup>38</sup>

Sedgwick comments: 'Sufficient! Yes, if the Creator has thought so: but has he so thought?' This is really the point. Sedgwick felt,

as a confirmed empiricist of the Baconian school, a dislike for the forms of natural theology which worked out a priori what God must

have done. It was in a similar vein that he had earlier said in

reviewing Chambers Vestiges: 'We dare not, like our author, go at

once to the great First Cause, and tell our readers what he MUST

have done ... in reasoning of creation we dare not, we repeat, tell

beforehand of what God must have done.'<sup>39</sup> Sedgwick's scientific

Baconianism and piety are linked. It is from actual empirical study

of the book of God's works and the book of God's word that we know

what he has done - not from a priori reasoning.'

Sedgwick also maintained his 'open' position on the question in a brief aside in his published 5th Edition Discourse around this time.

He says:

'But among the many million worlds, of which we see the germs in the sparkling atoms of the sky, organic creation may (for ought we know) be in continual activity; and the creative power may be shown continually, both in upholding laws that began in times past, and in ordaining laws which beforehand existed only in the prescient mind of God...'<sup>40</sup>

Sedgwick argues from humility. All creation takes place according to

an archetype pre-existing in the mind of God - but we see so little of that overall pattern from our own small part of the universe.

On a personal level, we may note, Sedgwick spoke in highest terms of both the persons and the work of Chalmers and Miller, but found it harder to like Brewster. He records his first meeting with Brewster: 'so sensitive and thin-skinned that you cannot touch him without making him wince.'<sup>41</sup> In 1835 Brewster wrote his Report in the Edinburgh Review on the first three meetings of the British Association. This presented a very slanted view of the origins and purpose of the BAAS, together with Brewster's assumptions about the decline of science (not popular with some of the Cambridge Dons, nor tactfully presented to them), and an irritatingly self glorifying insistence on his own importance in founding the Association. Thackray and Morrell in Gentlemen of Science have thoroughly analysed Brewster's role, and his controversy over the declinist issue, and there is no need to expand on these here. Sedgwick evidently found the article most irritating, and wrote to Phillips:

'It is clever and mischievous, full of vanity, being born of a selfish brain, half stifled and half repressed sentiments; and of the selfish, narrow and impracticable views of a man who with all his wonderful cleverness as an experimenter, is without true wisdom or moral dignity. These are hard words but I think he deserves them.'<sup>42</sup>

Mrs. Gordon in The Home Life of Sir David Brewster (1869) suggests that his commitment to Evangelicalism was more formal than typically Evangelical in devotional fervour during this earlier period.<sup>43</sup> If this is so, one can see why a man of a comparatively simple piety like Sedgwick would be drawn more to the kindred spirits of Chalmers and Miller than to Brewster. Brewster's apparent arrogance (though Brooke makes penetrating comment on this) would irritate Sedgwick,

and his way of thinking was along different lines. Surely, however, this cannot be traced to Brewster's Evangelical theology, but rather to the personalities of the two men. The totally different relationships between Sedgwick and Chalmers and Miller bear this out.

#### 4.3.3 Conclusions - Divergences in Natural Theology ?

No complex idea held in common by a group of people is likely to be viewed identically by all of them. It would therefore be naive to suggest that there were no differences between different natural theologians in the mid-nineteenth century. Amongst them there were, after all, people ranging from strict Evangelicals to liberals like the mature Powell. Yet the unanimity does seem to have been greater in this period than it had been (say) in the time of Paley. In Paley's day it could have been genuinely described as 'fragmented' - with Paley's moral philosophy (based on elements of his natural theology) at odds with that of Butler, Hall, etc. But, as we have seen, by the second quarter of the nineteenth century virtually all Christian groups had rejected this aspect of Paley and emphasized conscience as a natural theological basis for morality. Whilst some (like Whewell) linked this to a more idealist philosophy than others, the agreement is notable. It even finds the Evangelicals finding, to their agreeable surprise, much more in common with the liberal Brougham than one might have supposed. The plurality of worlds debate does not indicate any deep seated fragmentation of natural theology based on religious affiliations, but is an emergence of the two extreme views (in men neither of whom had any surfeit of humility, and who cordially disliked each other) within a broad spectrum of belief.

1. Whewell published anonymously (though the authorship was widely known) On the plurality of worlds: an essay (1853); Brewster wrote More Worlds Than One... (1854).
2. Brooke, 'Natural Theology and the Plurality of Worlds: Observations on the Brewster-Whewell Debate', Annals of Science, 1977, 34, 221-186.
3. Ibid., p. 262.
4. Ibid., p. 221, where he says their divergence 'can be correlated with the religious cultures to which they most closely belonged.'
5. Brooke says 'how precarious it would be to suggest that a Broad Churchman must have been wary of a plurality of worlds and all evangelicals enthusiastic advocates' (ref. 2), p. 251. But this caveat is weak in the context of the general tone of his article.
6. Brooke in Images of the Earth (Ed. L.J. Jordanova and R.S. Porter), p. 41: 'I have insisted elsewhere that to understand the plurality of worlds debate between Brewster and Whewell it is necessary to appreciate the respects in which the natural theology of a Scottish evangelical and that of a liberal Anglican could become so divergent as to become almost mutually exclusive.' See also Crosbie Smith, 'From Design to Dissolution', BJHS, 1979, 12, 59-70; Yeo, 'William Whewell, Natural Theology and the Philosophy of Science...', Annals of Science, 1979, 36, p. 506.
7. Brooke cites Cannon's article on this in (ref. 2), p. 232.
8. Brooke (ref. 2), p. 256.
9. Ibid., p. 231. Brooke also cites Brewster's signature of the 1865 Declaration as evidence of his conservatism, and notes Brewster's own association of this with his work on the plurality issue. This however, is an unfortunate example, as one of the two other signators identified as the most distinguished by W.H. Brock and R.M. Macleod in

- their article ('The "Scientists' Declaration"...', BJHS, 1976, 9, 39-66) was Adam Sedgwick (p. 52), who has usually been seen as a part of Cannon's supposed Broad Church network.
10. Brooke (ref. 2), p. 258.
  11. Chalmers, Discourses on the Christian Revelation, p. 41.
  12. First Impressions of England and Her People, p. 305.
  13. Ibid., p. 306.
  14. Brooke (ref. 2), p. 259.
  15. Ibid., p. 274.
  16. Ibid., p. 274.
  17. Ibid., p. 230 etc. Brooke's reference to Murchison's sympathy is not relevant as Murchison was not a liberal Anglican and, indeed, admitted to Sedgwick that his theistic beliefs stopped short of Christian faith (see C.R.Craig, 'Letters Concerning the Cambrian-Silurian Controversy of 1852', Jour. Geol. Soc., 1971, 127, 483-500).
  18. Brooke (ref. 2), p. 266.
  19. Cited in ibid.
  20. Edinburgh Review, July 1845, p. 63.
  21. Brooke (ref. 2), p. 266.
  22. Ibid.
  23. Ibid., p. 269.
  24. Ibid., p. 267.
  25. Yeo (ref. 6), p. 506.
  26. W.C. Hefferman, 'The Singularity of Our Inhabited World', J. Hist Ideas, 1970, 39, 81-100 (p. 86).

27. Though Brooke (ref. 2, p. 274) has that Miller 'sided with his fellow countryman' - perhaps meaning on a more limited issue.
28. Geology versus Astronomy, pp. 367-368.
29. Ibid., p. 379
30. Dated 4th March 1834, Trinity Whewell Collection, Add Ms 202,25.
31. Brooke (ref. 2), p. 258 n220, p. 280.
32. Ibid., p. 267 n283.
33. Clark & Hughes, 2, p. 269.
34. Handwritten note on p. 327 of Sedgwick's copy of On the plurality of worlds: an essay.
35. Handwritten note, ibid., p. 329.
36. Handwritten note, ibid., p. 330.
37. Handwritten note, ibid., p. 315.
38. Whewell (ref. 1), p. 314.
39. Sedgwick (ref. 20), p. 67.
40. Sedgwick, 5th Edn. Discourse, pp. ccxlii - ccxliii.
41. Clark & Hughes, 1, p. 265.
42. Letter dated 12th February in the Phillips collection the University Museum, Oxford.
43. See pp. 310-327.

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## 5.1 God and Nature

### 5.1.1 Introduction

With the development of a strong idea of 'natural law', questions arose for the Christian about the relationship between natural law and God. It was an early recognition of the orthodox that nature did not exist independently of God. Thus Buridan, discussing the possibility of God giving each of the 'celestial spheres' an initial impetus: 'has therefore no longer to move these spheres, except in exerting a general influence similar to that by which he gives his concurrence to all phenomena.'<sup>1</sup> The presence or concurrence of God, in some way, in created things, was also a theme taken up by Boyle and his contemporaries, in their very strong conception of 'natural law' which the 'mechanical philosophy' implied. Boyle speaks of the variety of creatures in the world as 'so many distinct engines', and says all these:

'as well as the rest of mundane matter are every moment sustained, guided, and governed, according to their respective natures and with an exact regard to the catholic laws of the universe ... there is a Being that does this every where and every moment, and That manages all things without aberration or intermission.'<sup>2</sup>

Boyle then goes on to speak of: 'those most signal and manifest interpositions we call miracles acted on by a supernatural way ...' He believed in the Biblical miracles, though stressing that miracles were 'very rare' events. In general:

'it seems very congruous to his wisdom to prefer ... catholic laws .. and uniformity in his conduct before making changes



in it according to every sort of particular emergencies; and, consequently, not to recede from the general laws He at first most wisely established to comply with the appetites or needs of particular creatures ... etc.'<sup>3</sup>

What is interesting is that Westfall, who cites the above passages amongst others of Boyle<sup>4</sup> does not seem to follow the basic consistency of Boyle's position. While Boyle spoke of 'interpositions', 'receeding from general laws' and 'changes in conduct', Westfall paraphrases to talk of 'violating the natural order' and 'establishing a machine that requires no intervention.'<sup>5</sup> So Westfall concludes: 'Boyle's opinion about miracles stood in absolute contradiction to the rest of his thought.' But this is simply not true. Whilst a Deist might think of the world as a machine created by God but running independently of him, a Christian like Boyle saw 'natural laws' as God's habits of conduct. A miracle on such a view is not an 'intervention' in an externally existing machine, but a change in pattern of behaviour which is usually uniform. As to the reason for it, Christian theists from Paley to C.S. Lewis have argued coherently on this - Paley, for example, saying that one would expect God to authenticate a revelation by miracles, and he would logically make miracles the more rare in order to make this authentication the more apparent.<sup>6</sup> What Westfall seems to be doing is to read back into Boyle a modern naturalistic view of natural law, rather than a Christian theistical one of God acting consistently in creation, upholding the world, and dealing with mankind; then he complains that grafting onto it a Christian idea of miracle is inconsistent.

These comments have been both to show the tradition into which Sedgwick fitted, and to sound a note of caution in interpreting it.

### 5.1.2 Sedgwick on God and Nature

Sedgwick states his views thus:

'God ordained natural laws, and while he continues to uphold them, material nature moves on in harmony; but were this sustaining power withdrawn, law would be at an end, and nature would relapse into confusion.'<sup>7</sup>

The phraseology of this is odd, reminding one of a Platonic concept that laws are somehow impressed on brute matter, and it does sound as though 'nature' has some kind of independent existence. But the meaning of the phrase is surely more likely to be that already given to it by Buckland in the following passage written in 1820:

'many who admit these proofs [i.e. of original design in creation] still doubt the continued superintendence of that intelligence, maintaining that the system of the Universe is carried on by the force of the laws originally impressed on matter, without the necessity of fresh interference or continued supervision on the part of the Creator. Such an opinion is indeed founded only on a verbal fallacy; for "laws impressed on matter" is an expression which can only denote the continued exertion of the will of the Lawgiver, the prime Agent, the first Mover: still, however... we perceive that the secondary causes producing these convulsions have operated at successive periods not blindly and at random, but with a direction to beneficial ends, we see at once the proofs of an overruling Intelligence continuing to superintend, direct, modify, and control the operations of the agents which he originally ordained..<sup>8</sup>

Sedgwick, in general, speaks only of God's 'creative power' and 'sustaining power'<sup>9</sup>, of the possibility of prediction because

'the Author of nature is unchanging',<sup>10</sup>. On miracles he follows Paley:

'If, on the other hand, we do believe in a God of nature, then can we conceive it possible, or even probable, that he should suspend what we call his laws, or manifest his power under some new form, to carry out the purposes of his will; and the question of a miracle thus becomes a question of fact to be decided on its proper evidence.'<sup>11</sup>

The laws, we note, are 'what we call his laws', to God they are just patterns of working. We shall see later that the view of God as 'sustainer', and natural laws only operating because of continual exertion of God's will, was a view shared by others of Sedgwick's scientific circle, when we look at the question of 'semi-deism'.

Sedgwick writes even more clearly in 1845:

'What know we of the God of nature (we speak only of natural means) except through the faculties he has given us, rightly employed, on the materials around us? In this way we rise to a conception of material inorganic laws, in beautiful harmony and adjustment; and they suggest to us the conception of infinite power and wisdom. In like manner, we rise to a conception of organic laws - of means (often almost purely mechanical as they seem to us, and their organic functions well comprehended) adapted to an end, - and that end only the well being of a creature endowed with sensation and volition.. Thus we rise to a conception both of Divine Power and Divine goodness; and we are constrained to believe, not merely that all material law is subordinate to His will, but that He has also (in the

way he allows us to see His works) so exhibited the attributes of His will as to show himself to the mind of man as a personal and superintending God, concentrating his will on every atom of the universe.'<sup>12</sup>

From this viewpoint, Sedgwick sees a basic division of 'miracles':

'There are two very distinct views of miraculous power. 1st, It may be shewn in the suspension or violation of a positive law of nature, long established and comprehended by experience. Or, 2ndly, it may be shewn in a new creative act; such, for example, as the formation of a new Class of animals, which may be in strict co-ordination with the established laws of nature, and therefore in a certain sense be no more miraculous than the generation of a new sentient being of a known species..<sup>13</sup>

Sedgwick goes on, of course, to say that there is the difference that a known creature is generated according to known laws, whereas we do not know any law for commencement of new species.

5.1 Notes

1. J. Buridan, Quaestiones Super Octo Libros Physicorum, Bk. 8.
2. Boyle, Of the High Veneration Man's Intellect Owe to God, pp. 140-1.
3. Boyle, Free Inquiry into the Vulgarly Received Notion of Nature, pp. 251-2.
4. Westfall, Science and Religion in Seventeenth Century England, pp. 85-88.
5. Ibid., p. 89. C.A. Russell, whilst still emphasizing the word 'Intervene', admits that 'implicit' in Boyle is the position that 'God is the immediate as well as the ultimate cause of all phenomena.' (Science and Social Change 1700-1900, p. 46).
6. See above, especially part 3.3.1.
7. 5th Edition Discourse, p. cclxxix.
8. W. Buckland, Vindiciae Geologicae, pp. 18-19.
9. 5th Edition Discourse, p. xv.
10. The Athenaeum, October 5th, 1844.
11. 5th Edition Discourse, p. ccxxx.
12. Edinburgh Review, July 1845, p. 63.
13. 5th Edition Discourse, p. ccxxx.

## 5.2 The Background of Geology

### 5.2.1 Background and the Eighteenth Century

During Sedgwick's lifetime the most important treatment of geological history was in Lyell's Principles, published in the 1830s. Whewell's History of the Inductive Sciences (1837) seems to have relied heavily upon it, as did most of the following histories. A landmark in the modern study of that history must be Gillispie's Genesis and Geology (1951), followed by contributions by Cannon and Hooykaas in the early 60's, Rudwick and Porter and others in the 70's. Many of the earlier misconceptions have been corrected by these later works, so only a very brief and unoriginal background sketch will be attempted here.

The late 18th century saw a situation of hostility between the rival schools of Werner (sometimes called 'Neptunists') and of Hutton (sometimes called 'Vulcanists'). The precise differences between their systems do not concern us here, except for one feature. This was that the system of Hutton was a 'steady state' theory. The agency of rivers and erosion constantly deposited new material in the sea, forming new strata; whilst simultaneously the action of internal heat forces caused new land masses to rise out of the water. The geological cycle is therefore endless, and 'The result, therefore, of our present enquiry is, that we find no vestige of a beginning, - no prospect of an end.'<sup>1</sup> It was not, of course, that Hutton or his disciple Playfair said that there had been no beginning, but simply that science could discover no trace of one.

### 5.2.2 Geology in the Early Nineteenth Century

The nineteenth century saw three main developments. The first was the establishing of the Geological Society of London in 1807 which saw its task to leave aside speculation and concentrate on empirical

research. The second was the publication in 1815 of William Smith's map of the 'Strata of England and Wales' (though Wales is actually nearly all the same colour!). But his method of dating strata by fossils came soon to be widely accepted. The third was the publication of Cuvier's Discours sur les revolutions de la surface du globe in 1811, with other works. In the Paris Basin Cuvier had found alternate layers of alluvial deposits, and concluded that there had been a series of universal floods. The work was translated by Jameson in 1817, but with an added emphasis on the identification of the last of these floods with the Noarchian one. This was taken up by the newly appointed Reader at Oxford William Buckland in his Vindiciae Geologicae (1820). Sedgwick adopted the same basic idea, along with Wernerian ideas about the deposition of strata.<sup>2</sup> Not everyone accepted this. In Scotland Dr. John Fleming argued that the idea of such catastrophic revolutions was against both the evidence and Scripture.<sup>3</sup> On the continent, Scrope and Prevost argued against it in different ways.<sup>4</sup> Sedgwick apparently lost his diluvial views in 1829. Lyell wrote to Fleming: 'Sedgwick and Murchison are just returned, the former full of magnificent views. Throws overboard all the diluvian hypothesis; is vexed he ever lost time about such a complete humbug, says he lost two years by having started a Wernerian...'<sup>5</sup> His address to the Geological Society in taking over as President in 1830 contained an emphasis that many different origins could exist for valleys, and that in some instances the elevation of land masses was the only explanation.<sup>6</sup> On retiring from the post in 1831 his address contained a clear recantation of his former diluvian views.<sup>7</sup> In the previous year the first volume of Lyell's Principles had come out, arguing against both diluvialism and any form of alteration in the rate or kind of forces forming

geological strata. Following Sedgwick's review of this in his Presidential Address to the Geological Society, Fleming promptly wrote to him 'as an honest enquirer after truth', complaining at lack of recognition for priority in Lyell's uniformitarian principles.<sup>8</sup> But, new or not, the ideas were gaining ground. Buckland soon followed Sedgwick, and in 1836 quietly withdrew his diluvialism in a footnote.<sup>9</sup>

Now in the same Presidential address of 1831 in which he read his recantation, Sedgwick reviewed Lyell's Principles. He accepted 'nineteen twentieths' (p.311) of the work, but had one major criticism. Lyell had assumed not merely that the same kind of forces in operation now should be used to explain past phenomena, but that these operated at the same intensity throughout geological history. This, in Sedgwick's view, would be acceptable if based on actual observation, but in fact it has been made as a purely arbitrary assumption.

Diluvialism was dead. But there remained a new form of controversy between those called 'uniformitarians' and those called 'catastrophists'. Although much has been written in recent years to clarify the issues here, it is still possible to find in a recently published book a statement that Whewell believed 'God intervened in the course of nature with catastrophic alterations to its geology and complement of species.'<sup>10</sup> It may, therefore, be worth outlining the real issues here.

- (a) Actualism: This was the belief that forces now in operation should be used to explain observations made of evidences showing past alterations.
- (b) Uniformitarianism: This was the belief that not only the same forces had always operated, but that they had operated at the same intensity.



- (c) Steady State Theory: This was the belief that there was no sign of any kind of progressive change in the geological past, i.e. there was no trace of any beginning to the process.

There has to be a distinction made in examining attitudes on these issues, between the respective spheres of geological history (i.e. rock formation) and organic history. On the geological history, both the Lyellian uniformitarians and the catastrophists agreed on (a), but differed on the other two points. Thus, for example, Sedgwick in his 1831 Presidential address reviewed Lyell's Principles and said that he accepted 'nineteen twentieths' of its material.<sup>11</sup> But he went on:

'According to the principles of Mr. Lyell, the physical operations now going on, are not only the type, but the measure of intensity of the physical powers acting on the earth at all anterior periods: and all we now see around us is only the last link in the great chain of phenomena, arising out of a uniform causation, of which we can trace no beginning, and of which we can see no prospect of the end. And in all this there is much that is beautiful and true. For we all allow, that the primary laws of nature are immutable - that all we now see is subordinate to those immutable laws - and that we can only judge of effects which are past, by the effects we behold in progress... But to assume that the secondary combinations arising out of the primary laws of matter have been the same in all periods of the earth is an unwarrantable hypothesis with no a priori probability, and only to be maintained by an appeal to geological phenomenae...'<sup>12</sup>

In a letter dated 7th March 1837<sup>13</sup> Lyell complained to Whewell that

Sedgwick had misrepresented him. He claimed firstly that he had made an exception for man (which was true), and also that he had advocated uniformity only as an approach to scientific research. There is no necessity for our present purpose to go further into the precise differences between catastrophism and uniformitarianism<sup>14</sup>. Certainly Sedgwick thought Lyell had arbitrarily assumed point (b) with regard to rock formation, and that (c) was unlikely. It ignored, for example, the cooling of the earth.

On the organic question, the divergence was even more marked. Here Sedgwick and his circle (Buckland, Conybeare, Murchison, Whewell and practically every one else) denied all (a), (b) and (c). They knew of no process now going on which could account for the sudden appearance of new species in the strata. In 1845 Sedgwick wrote to Agassiz:

'I say we have successive forms of animal life adapted to successive conditions (so far proving design), and not derived in natural succession in the ordinary way of generation. But if no single fact in actual nature allows us to suppose that the new species and orders were produced successively in the natural way, how did they begin? I reply by a way out of and above common, known, material nature, and this way I call creation.'<sup>15</sup>

In actual fact Lyell himself seems to have been vague about the mechanism of the sudden appearance of new species - for he rejected the then current theories of evolution no less than his critics. But what marks him out as what Bartholomew has called a 'singular figure' was his view that apart from man no new kind of genera had appeared throughout geological history.<sup>16</sup> There had been no

kind of progression of animal forms, and the absence of higher forms from the earlier strata were to be explained because of the rarity of conditions for fossilising.

It is one of the strange ironies of geological history, that in spite of the often repeated assertion that Lyell (or Hutton or both) "founded modern geology" and that it is "based on uniformitarianism", there was actually no major issue on which he was right and his opponents were wrong! But that is not really part of our present concern.

## 5.2 Notes

1. Theory of the Earth, p. 304.
2. In 1825 he wrote two letters to the Annals of Philosophy, 'On the Origin of Alluvial and Diluvial Formations.'
3. The Edinburgh Philosophical Journal, 1825, 12, 116-127 and 1825-26, 14, 205-239.
4. See Rudwick, The Meaning of Fossils, ch. 3, etc.
5. Life, Letters and Journals of Sir Charles Lyell, Bart (Ed. Mrs. Lyell) 1, p. 256; also Clark & Hughes, 1, p. 357.
6. Proceedings, 1830, 1, 187-212.
7. Proceedings, 1831, 1, 281-316.
8. Letters IA44 also IA44A, Sedgwick Collection University of Cambridge, dated 15th November and 23rd December 1831. The extent of Lyell's originality has been much discussed by Bartholomew, Rudwick, Cannon etc. in their various articles and papers. See also Pietro Corsi, 'The Importance of French Transformist Ideas for the Second Volume of Lyell's Principles of Geology', BJHS, 1978, 9, 221-244.
9. Geology and Mineralogy Considered With Reference to Natural Theology, pp. 94-95n.
10. C. Chant and J. Fauvel (Eds.), Darwin to Einstein: historical studies on science and belief, p. 77.
11. Sedwick's Address is in Proc. Geol. Soc. Lond., 1831, 1, pp. 281-316. (ref. p. 311).
12. Ibid., p. 304.
13. Lyell (ref. 5), 2, pp. 2-7.

14. See e.g. Hooykaas, The Principle of Uniformity in Geology, Biology and Theology, pp. 1-47, and Rudwick, The Meaning of Fossils, ch. 3.
15. Clark & Hughes, 2, p. 86, also in Louis Agassiz, His Life and Correspondence, 1, p. 383.
16. See Bartholomew, 'The Singularity of Lyell', Hist. Science, 17, 276-293; Bartholomew, 'Non-Progress of Non-Progression...', BJHS, 1976, 9, 166-174; Cannon, 'Charles Lyell, Radical Actualism and Theory', BJHS, 1976, 9, 104-120; the issue is mentioned in various other sources; see also Rudwick's 'A Critique of Uniformitarian Geology', Proc. Amer. Phil. Soc., 1967, 3, 272-287, on Conybeare's criticisms.

### 5.3 Geology and Natural Theology

#### 5.3.1 The Background Issue

A natural theology argument from design could be made at two levels:

- (i) Design of any kind, even if it has always been there, implies the existence of a designer.
- (ii) Design in an object which is known at one time not to have existed, implies the existence of a designer to make it.

Careful study of Paley's argument shows that he, at least, actually argued (i) but the psychological force of the argument depended on (ii). In chapter 1 section iv of Natural Theology he says:

'Nor is there any thing gained by running the difficulty further back, i.e. by supposing the watch before us to have been produced from another watch, that from a former, and so on indefinitely. Our going back ever so far brings us no nearer to the least degree of satisfaction upon the subject. Contrivance is still unaccounted for. We still want a contriver ...'

Paley argues that to increase the causal chain to an infinite one makes no difference. The whole series still retains marks of design, and this requires a designer. This is perhaps interesting in the context of the later Darwinian debate - for strictly logically Paley's design argument is left by the evolutionary hypothesis exactly where it was before.<sup>1</sup>

It must be recognised, however, that the psychological force of Paley's argument probably stemmed not from (i) but from (ii). The force of the analogy comes in an implicit assumption or feeling in our minds that the watch arose or began at some point in time, and order and purpose was introduced into a situation of none.

### 5.3.2 The Contribution of Geology

Now the 'steady state' theory of Hutton (or for that matter of Lyell) gave no firm indication that there was such a beginning in time for the causal sequence of reproduction as we know it. The evidence as read by the catastrophists did. Thus Buckland in 1820 wrote:

'When our minds become thus familiarised with the idea of a beginning and first creation of the beings we see around us, the proofs of design, which the structure of those beings affords, carry with them a more forcible conviction of an intelligent Creator, and the hypothesis of an eternal succession of causes is thus at once removed. We argue thus - it is demonstrable from Geology that there was a period when no organic beings had existence: these organic beings must therefore have had a beginning subsequently to this period; and where is that beginning to be found, but in the will and fiat of an intelligent and all-wise Creator?... if some writers on Geology in later times have professed to see in the earth nothing but the marks of an infinite series of revolutions, without the traces of a beginning; it will be quite sufficient to answer that such views are confined to those writers who have presumed to compose theories of the earth, in the infancy of the science, before a sufficient number of facts had been collected...'<sup>2</sup>

If geology goes further and shows that the 'present system of this planet is built on the wreck and ruins of one more ancient, there is nothing in this inconsistent with the Mosaic declaration.' There was no need for Moses' purpose to tell us of these things. Buckland backs this view up by quoting Sumner 'a divine whose rational and sober piety no person will venture to dispute.'

In 1836, although no longer a diluvialist, he can still write:

'Geology has rendered an important service to Natural Theology, in demonstrating by evidences peculiar to itself, that there was a time when none of the existing forms of organic beings had appeared on our Planet, and that the doctrines of the Development and Transmutation from other species, or by an Eternal Succession from preceding individuals of the same species, without any evidence of a Beginning or prospect of an End..<sup>3</sup>

Sedgwick, in his Discourse, takes the proof from contrivance in two distinct ways. The first<sup>4</sup> he argues 'addresses the imagination':

'It is certain that the glories of the external world are so fitted to our imaginative powers as to give them a perception of the Godhead, and a glimpse of his attributes ... The heavens declare the glory of God and the firmament sheweth his handy work. Here is a direct assertion - an appeal to the heart, and not to the understanding..<sup>5</sup>

But the appeal to the reason is different from this. It sees that 'contrivance proves design in every organic being we survey.'<sup>6</sup>

Sedgwick continues:

'It is in vain that we attempt to banish an intelligent Creator, by referring all changes organic and inorganic, to a succession of constant material actions, continued during an eternity of past time. Were this true, it would not touch our argument: and every instance of organic contrivance or material adaptation, would be a phenomenon unexplained, except on the supposition of a contriver. It would only prove that, in a certain portion of space, God had thought fit to give a constant manifestation of his wisdom and power through an indefinite period of duration.



The eternity of material forms is, however, but a dream of vain philosophy, unfounded in reason or analogy; and at least as far as organic nature is concerned, contradicted by the plainest physical records of the past world.<sup>7</sup>

'Geology gives its aid to natural religion ... It tells us that God has not created the world and left it to itself, remaining ever after a quiescent spectator of his work: for it puts before our eyes the certain proofs, that during successive periods there have been, not only great changes in the external conditions of the earth, but corresponding changes in organic life ... It shews intelligent power not only contriving means adapted to an end: but at many successive times contriving a change of mechanism adapted to a change of external conditions; and thus affords a proof, peculiarly its own, that the great first cause continues a provident and active intelligence.'<sup>8</sup>

So did Sedgwick at this time believe that the demonstrations (as he saw them) of geology were essential to Christianity? This seems highly unlikely. His own Christian faith predated both his own knowledge of geology, and any general realisation in scientific circles that fossils showed species not to have been eternal. As we have seen, even the Paleyan argument from design does not depend strictly on an assumption of geological demonstrations of beginnings for species. As for demonstrations of God's continuing interest in his creation, Sedgwick says only that geology offers 'a proof, peculiarly its own..' Other proofs (e.g. miracles and prophecy) he would not have denied. Conscience would have remained a key concept in the natural theology of a moral God. Thus the geological demonstrations of species creation in the past ages was essential neither to the revealed religion of

Christianity nor even to important elements of his natural theology.

We have seen that the modern tendency to place Sedgwick in a supposed 'Broad Church' is misleading, and that on essential points his views were far closer to the moderate Evangelicals like Simeon and Chalmers. But there have been other ascriptions of views to Sedgwick on natural theology and creation which have been even more misleading. In particular there is the contrast made by both Hooykaas and Gillispie between Sedgwick and the important Scottish Evangelical geologist Hugh Miller. In actual fact Miller's attitude to geology and natural theology was virtually identical to Sedgwick's. Writing of his first impressions on a visit to England in 1844 he commented:

'There is, I doubt not, a day coming when writers on the evidences of the two Theologies, Natural and Revealed, will be content to borrow largely from the facts of the geologist... Infidelity has toiled hard to obviate the necessity of a First Great Cause, by the fiction of an Infinite Series; and Metaphysic Theology has laboured hard, in turn, to prove the fiction untenable and absurd. But metaphysicians, though specially assisted in the work by such men as Bentley and Robert Hall, have not been successful. They have, indeed, shown that an infinite series is, from many points of view, wholly inconceivable, but they have not shown that it is impossible... Metaphysic Theology furnishes no real argument against the "Infinite Series" of the atheist. But Geology supplies the wanting link.'<sup>9</sup>

Miller returns to this theme a number of times elsewhere, e.g. in The Testimony of the Rocks ch.5., published in 1857. Here he cites Paley as well as Robert Hall and Bentley as providing insufficient argument against the infinite succession idea.<sup>10</sup> After speaking of

Paley he says:

'Geology takes up the master volume of the greatest of the natural theologians, and after scanning its many apt instances of palpable design, drawn from the mechanism of existing plants and animals, authoritatively decides that not one of these plants or animals had begun to be in the time of the Chalk... In fine, metaphysic geology furnishes no argument against the infinite series of the atheist. But geology does.'<sup>11</sup>

Here we find the same kinds of influence, particularly Robert Hall, and the same insistence as in Sedgwick that now geology has enabled natural theology to go a step further.

Whewell, incidentally, though less explicit (as one might expect) than the specialists in geology, advances similar views. Reviewing the first volume of Lyell's Principles, he notes that Lyell's 'steady state theory' makes no attempt to account for the arrival of new species:

'to give even a theoretical consistency to his system, it will be requisite that Mr. Lyell should supply us with some mode by which we may pass from a world filled with one kind of animal forms, to another, in which they are equally abundant, without perhaps one species in common ... We conceive it undeniable (and Mr. Lyell would probably agree with us) that we see in the transition from an earth peopled by one set of animals, to the same earth swarming with entirely new forms of organic life, a distinct manifestation of creative power, transcending the known laws of nature; and it appears to us, that geology has thus lighted a new lamp along the path of natural theology.'<sup>12</sup>

Gillispie<sup>13</sup> saw this as a 'lust for the catastrophic and miraculous',

and Cannon<sup>14</sup> criticised him for this and pointed out that the word 'creative' is also used by Darwin and Lyell and does not imply the supernatural. This is true, but Cannon omits the last clause about geology lighting a new lamp along the path of natural theology. There seems no likely explanation of this clause except that Whewell has in mind a similar argument to that of Sedgwick et al. Whewell says that the manifestations 'transcend' known laws, which does not seem to imply that he expected extensions of these to explain them. In 1837 in his History of the Inductive Sciences Whewell spoke of the 'impossibility of accounting by any natural means for the production of all the successive tribes of plants and animals which have peopled the world in its various stages, as geology teaches us... but when we enquire whence they came into this our world, geology is silent. The mystery of creation is not within the range of her legitimate territory; she says nothing, but she points upwards.'<sup>15</sup> Cannon's comment that in 1831 'Whewell had not allied himself with the miraculous', and that later he 'committed the Catastrophists to the task of furnishing at least a description of what a miracle looks like to a scientist',<sup>16</sup> does not seem to be a plausible interpretation of these passages. Whewell does remain vague about the nature of a 'creative' act, but his early phrase 'transcending ...', and his insistence that creation is 'not within the legitimate territory' of geology seem to indicate that some kind of miraculous event is in mind.

1. This is not to deny P.J. Bowler's distinction ('Darwinism and the Argument from Design', JHB, 1977, 10, pp. 29-43) between arguments from design based on 'adaptation of individual species to their environment' and 'idealist concepts of design, based on the overall pattern of creation' (p. 30). To Bowler, Owen typifies the latter, and Agassiz and Miller are mentioned, but Sedgwick (who certainly belongs with this group) is strangely omitted. Natural selection (though not Chambers' evolution) offers an alternative 'accidental' explanation for origins of particular adaptations given the physico-chemical structure of the universe. But it leaves (as with Paley) the ultimate 'design argument' question of why its structure is such as to lead to such phenomena. Sedgwick, incidentally, criticised Buckland's Bridgewater for applying the design argument to detail rather than overall pattern (Clark & Hughes, 1, pp. 469-471).
2. Vindiciae Geologicae, p. 21.
3. Geology and Mineralogy Considered with Reference to Natural Theology.
4. Sedgwick, 1st Edn. Discourse, pp. 17-18.
5. Ibid., p. 18.
6. Ibid., p. 19.
7. Ibid., p. 20-21.
8. Ibid., p. 23.
9. First Impressions of England and Its People (14th Edn.), p. 302.
10. The Testimony of the Rocks, p. 176.
11. Ibid., pp. 176 and 178.
12. British Critic, 1831, 9, p. 194, cites this.
13. Gillispie, Genesis and Geology, p. 146.

14. Cannon, 'The Problem of Miracles in the 1830's', Victorian Studies, 1960-61, 4, p. 8.
15. Whewell, History of the Inductive Sciences, part 3, p. 488.
16. Cannon (ref. 14).

## 5.4 The Meaning of Creation

### 5.4.1 Sedgwick on Creation

We have seen Whewell's vagueness about acts of creation; what now of Sedgwick ? There seems no discernible movement in Sedgwick's views on either the nature or chronology of creation during his long life. He did change his mind on the antiquity of man (at the incredible age of 83 ),<sup>1</sup> but this was not central.

Sedgwick held that:

'... before the creation of all worlds, there was an archetype of Nature (dead as well as living, past as well as present) in the prescient mind of God.'<sup>2</sup>

In Sedgwick's view it was this which explained the similarities in anatomy of different species - not (as the evolutionists argued) a common ancestry. Sedgwick held a view of successive creations, quoting approvingly from Agassiz on the matter:

'Il faut nécessairement remonter à une cause plus élevée, et reconnoître des influences plus puissantes, exerçant sur la nature entière une action plus directe, si l'on ne veut pas se mouvoir éternellement dans une circle vicieux. Quant à moi, j'ai la conviction que les espèces ont été créées successivement à différentes reprises ...et que les changemens qu'elles ont subis durant une époque géologique ne sont que très secondaires, et ne tiennent qu'à leur plus ou moins grande fécondité, et à des migrations subordonnées à des influences de l'époque.'<sup>3</sup>

Translated this reads:

'It is necessary to ascend to a higher cause, and recognise more powerful influences exerting on the whole of nature a more direct action, unless one is to enter an eternal vicious circle.

As for me, I am convinced that species have been created repeatedly and successively ... and that the changes which they have undergone during any one geological epoch are no more than very secondary and are related only to their greater or lesser fecundity and to the migrations resulting from the influences of the period.'

He also wrote to Agassiz on April 10th 1845, and Agassiz replied in June. In his reply he stated:

'I find it impossible to attribute the biological phenomena, which have been and are still are going on upon the surface of our globe, to the simple action of physical forces. I believe that they are due, in their entirety, as well as individually, to the direct intervention of a creative power acting freely and in an autonomic way... the differences between animals do not constitute a material chain, analogous to a series of physical phenomena, bound together by the same law, but present themselves rather as the phases of a thought, formulated according to a definite aim. I think we know enough of comparative anatomy to abandon forever the idea of the transformation of the organs of one type into those of another.'<sup>4</sup>

Sedgwick, in the 5th Edition of his Discourse pages xciv to cxxviii, outlines the evidence to show that Fish, Reptiles, Birds and Mammals were created - often the highest forms of the class appearing suddenly and first in the record. He says, e.g.:

'How then were Birds called into being? We reply that they were created. The word expresses the exact condition of our knowledge. It assumes the existence of an Intelligent Power in nature acting with prospective wisdom: and it tacitly affirms, as a negative fact, that we comprehend not the manner in which this form of organic life began. It was exactly like reasoning from the



Class of Fishes that Agassiz arrived at a similar conclusion...<sup>5</sup>

'These Mammals... have no zoological base to rest upon. They were therefore not called into being by any known law of nature, but by a power above nature. They were created by the hand of God, and adapted to the conditions of the period. This is the conclusion of Agassiz and Owen on better evidence than Cuvier possessed; and this was, in substance, the grand conclusion of Cuvier.'<sup>6</sup>

On the actual mechanism of creation we can, he says, know nothing:

'In one view we see the great animating First Cause in the laws impressed by Him on the vast bodies of the material universe. In another view we see Him in positive acts of creative power shewn in the organs of successive animated beings brought into life during long successive periods. ... We know that God has created: but how he creates, we can form not the least conception; for external nature is known to us only through its established laws, and their manifestation to our senses; and these laws imply a past, not a present creative act. It may be that in the mind of God there is no greater or different exercise in Creative Power in the continuance of a law than in its commencement. But it is not so in the mind of man ...'<sup>7</sup>

In other words, today we can only observe laws operating which God set up in the past - we cannot observe fresh creative acts. Therefore we cannot form a conception of how He does them. This does not concern Sedgwick unduly, for he recognises the time-bound nature of our minds, and does not think we need to know much about Creative acts. He is quite clear, however, that these acts are of a different order from the ordinary succession of second causes. He states this in a speech reported in the Athenaeum of 1844, and also thus in a letter to Agassiz already mentioned:

'But if no single fact in actual nature allows us to suppose

that the new species and orders were produced successively in the natural way, how did they begin? I reply, by a way out of and above common known, material nature, and this way I call creation. Generation and creation are two distinct ideas, and must be described by two distinct words ...'<sup>8</sup>

There are positive and negative aspects of the word creation. The negative aspect is that our knowledge of God's dealings through nature's laws:

'... gives us no conception of the mysterious operations of his everlasting will, which moved him either to create or to withhold his creating power.'

In the positive sense:

'the word creation ... implies our belief in a fact; and it also implies our belief that such fact was the offspring of prescient thought and design - it implies, in one word, the personality of the Godhead.'<sup>9</sup>

'We know that God has created: but how he creates, we can form not the least conception ... The movements of his Will are the beginnings of every material cause, and these movements have no limitations in time and space. It is then utterly beyond the reach of human thought to comprehend what creation means as contemplated in the mind of God. But we do see the proofs of a creative energy within that spark of the universe wherein we dwell; and these acts of energy are defined in time and limited in space or they could not be the objects of our contemplation.'<sup>10</sup>

In 1860, in his anonymous review of Darwin's Origin of Species, Sedgwick's position seems unchanged. He says:

'Species have been constant for thousands of years ... Change

the condtions and old species would disappear; and new species might have room to come in and flourish. But how, and by what causation? I say by creation. But what do I mean by creation? I reply ... a power I cannot imitate or comprehend; but in which I can believe, by a legitimate conclusion of sound reason drawn from the laws and harmonies of Nature. For I can see in all around me a design and purpose, and mutual adaptation of parts, which I can comprehend - and which prove that there is exterior to, and above, the mere phenomena of Nature a great prescient and designing cause. Believing this I have no difficulty in the repetition of new species during successive epochs in the history of the earth.

But Darwin would say I am introducing a miracle by the presupposition. In one sense, I am; in another I am not. The hypothesis does not suspend or interrupt an established law of Nature. It does suppose the introduction of a new phenomenon unaccounted for by the operation of any known law of Nature; and it appeals to a power above established laws, yet acting in harmony and conformity with them.' 11

The language here reflects the distinction made between the two kinds of 'miracle' in the Discourse (cited above, p. ccxxx). There seems to be no change of view. At the same time Sedgwick wrote to Owen:

'I want to learn your views about Creation's law. It is clear that there has been a law governing the succession of forms. But here, by law I mean order of succession, and not a law like that of gravitation, out of which the actual movements of our system follow by mechanical succession. In that sense I do not believe in any law of creation. The highest point we can, I think, ever reach is a law of succession of forms, each implying a harmonious reference to an archetype and each having indications of the action of a

final cause - i.e. of intelligent causation or creation...<sup>12</sup>

What Sedgwick seems to mean is that with hindsight we can see how God is relating all the forms to archetypes (this was an idea developed by Owen and Agassiz in particular), but (unlike gravitational laws) it would not enable us to predict when, where, and what would be created next. There seems, then, some movement of view from his earlier ones. But it is more a belief that we can discern some pattern in God's working (and surely Sedgwick would never have suggested that he had no such pattern, only that we could not know it); it is not a belief that natural law as such could comprehend it. A similar idea seems to be implicit in Hugh Miller's words:

'The foreknown archetypal idea of Owen, - "the immaterial link of connection" of all the past with all the present, which Agassiz resolves into the fore-ordained design of the Creator, - will yet be found, I cannot doubt, to translate themselves into one great general truth, namely, that the Palaeozoic, Secondary and Tertiary dispensations of creation were charged, like the patriarchal and Mosaic dispensations of grace, with the "shadows of better things to come."<sup>13</sup>

This, indeed, raises the question of the relation of the ideas of Sedgwick (and Agassiz and Miller) to those of Owen. Adrian Desmond, in Archetypes and Ancestors, has analysed Owen's ideas in relation to the context of the times. Desmond sees the basic conflict as being being a Platonic idealism (as of Owen), and an implied materialism (as of Huxley). My own perception of the situation, though arrived at independently, would be along similar lines, though with the obvious proviso that to be a member of the Darwin circle was not necessarily to verge on a materialistic monism in the manner of Chambers. Those who might sympathise with Owen were in fact a wide and distinguished group.<sup>14</sup> Clearly, however, there were differences between Sedgwick and

Owen, but the precise analysis of these is hampered by the obscurity of what exactly Owen did believe - in spite of Desmond's recent work. Desmond takes Owen's attitude to the Vestiges, and his comment on 'sacerdotal revilers' in the review of the Origin, both to relate to a belief in creation by law.<sup>15</sup> But the only actual hint Owen seems to give of this is reference to metagenesis.<sup>16</sup> Paradoxically, however, Owen (who was no geologist) differed from Sedgwick in minimising the fossil gaps,<sup>17</sup> whilst if the mechanism really were analogous to metagenesis then presumably very large 'gaps' could be accommodated. He also seemingly differed from Sedgwick in emphasizing the continued working of the creative process,<sup>18</sup> though no details are given of new species, and it is unclear as to how this relates to the arrival of man in a privileged position. Finally, if Desmond is right in suggesting that Owen reacted against the Origin because of an association with materialism<sup>19</sup> rather than its inherent implications, this would make his reaction analogous to that of Sedgwick. Further comparison is made too difficult by lack of sufficient detail of information.

#### 5.4.2 Sedgwick a 'Nomothetic-creationist' ?

There is much interesting comment on the meaning of 'special creation' and its relationship with Darwinianism in a recent book by Neal C. Gillespie Charles Darwin and the Problem of Creation. His treatment of Sedgwick on this question, however, is puzzling. He gives a list of those mentioned by Darwin in 1859 as being 'foes of the mutability of species', including Agassiz and Sedgwick. Then he comments: 'not one, with the exception of Agassiz ... believed in miraculous creation in 1859'.<sup>20</sup> He later says: 'Close to the still powerful biblical tradition in conceptualisation, and, usually, in personal religious faith, were those who felt confident that the evidence of science confirmed their belief in a miraculous creation ...'<sup>21</sup> In the late fifties he classes with these Miller and Agassiz - Sedgwick

is notable by his absence. Then later he says: Sedgwick: 'exceeded most nomothetic creationists in his nescience as well as in his divergence towards miracle...' following this with quotations from the 5th Discourse.<sup>22</sup> Why is Agassiz but not Sedgwick said to believe in miraculous creation and in science's confirmation of this? Why is Sedgwick classed with the 'nomothetic creationists' (i.e. who believe creation is by laws)? Surely his comments to Owen (all that Gillespie cites) do not so mark him? Agassiz himself in an earlier reply to Sedgwick, said:

'I would even say that I believe the creation of man has closed creation on this earth, and I draw this conclusion from the fact that the human genus is the first cosmopolite type in Nature. One may even affirm that man is clearly announced in the phases of organic development of the animal kingdom as the final term of this series.'<sup>23</sup>

Sedgwick was certainly no more nomethetic than Agassiz. Gillespie follows this with some misleading comments about the methodology of Sedgwick (and others) and only just seems to conclude that the work of one of the generally acknowledged best field geologist of his generation cannot be treated 'merely as a form of theology or biblical criticism'. Then he finishes by saying that the 'nescient approach to the problem of origins' is in a sense 'a halfway point on the road to a fully positivistic view of biology'. Even aside from the ambiguous way in which he has defined the term 'positivist', this seems unlikely. Sedgwick only declines the term 'miracle' because he defines two senses for the term - he is no nearer 'positivism' than Agassiz. Miller, it is true, seems in one place to imply that creation of many fish species was instantaneous.<sup>24</sup> But how central this really was to his approach seems uncertain.

One other interesting point concerns Lyell. His modern biographer has recorded how the reader of the Principles 'is left entirely in suspense' about the question of how new species might arise in his steady state system.<sup>25</sup> Contemporaries like Whewell noticed the same thing. In a now famous correspondence Herschel wrote to him in 1836 from Capetown suggesting that God might operate in replacing extinct species by others 'through a series of intermediate causes & that in consequence, the origination of fresh species ... would be found to be a natural in contradistinction to a miraculous process...'<sup>26</sup> Lyell replied saying that he 'left this to be inferred' from his work. Now Gillespie says of this:

"natural," I would suggest, meant to (Herschel), not positivistic or secular causes as it would today, but rather nomothetic creationism: "natural" in the sense of being within nature, but not divorced from theological causality.<sup>27</sup>

This is curious. Surely to any theist, all natural causation (however invariant) is ascribed to the ultimate agency of God? Yet on page 153 he says: 'Scientists who were theists could also be positivists.' So what is the difference between a nomothetic creationist and a theistic positivist? Both might believe that the natural law sequence was invariant, and both surely would believe that invariant natural law sequence to be ultimately ascribed to God? Were one to describe Lyell and Herschel, at this stage, in Gillespie's terms one would call them 'nescient theistic positivists'. The fact that he does not, seems to reflect a confusion in his terms, in particular in the meaning of the word 'positivist', but further exploration of this would be beyond our present scope in dealing with Sedgwick.<sup>28</sup>

5.4 Notes

1. Clark & Hughes, 2, p. 440.
2. 5th Edition Discourse, p. ccxix.
3. Cited in Sedgwick's review of the Vestiges p. 56, where quane is printed instead of quant, 5th Edition Discourse, p. xcvi.
4. Printed in Louis Agassiz, His Life and Correspondence, 1, p. 389.
5. 5th Edition Discourse, p. cv.
6. Ibid., p. cxiii.
7. Ibid., p. ccxxxviii.
8. Clark & Hughes, 2, p. 85, Agassiz (ref.4), p. 385.
9. 5th Edition Discourse, p. cxxxvi.
10. Ibid., p. ccxxxvii-cxxi.
11. Spectator, April 7th, 1860.
12. Clark & Hughes, 2, p. 361.
13. The Testimony of the Rocks, p. 196.
14. Rupke, in The Great Chain of Being, also notes the 'idealism' of Owen, but restricts it to Agassiz and Miller because of his attempt to magnify a polarisation into the 'Oxford' and 'Edinburgh' schools.
15. Desmond, Archetypes and Ancestors, pp. 31-32, 56.
16. Ibid., p. 35 etc.
17. Ibid., p. 80.
18. Ibid.
19. Ibid., pp. 60-61.
20. Gillespie, Charles Darwin and the Problem of Creation, p. 26.
21. Ibid., p. 27.



22. Ibid., p. 29.
23. Louis Agassiz, His Life and Correspondence, 1, p. 394.
24. Miller, Footprints of the Creator, p. 292.
25. See Wilson, Charles Lyell, the Years to 1841, p. 339.
26. Cited in Ibid., p. 439, and elsewhere.
27. Gillespie (ref. 15), p. 31.
28. J.R. Moore, in a long review of Gillespie (BJHS, 1981, 14), is also critical of the contradictions in the book. He also recognises its 'positivist or whiggish standpoint' (p. 192), and the author's failure to reckon sufficiently with the 'historically long lived integration of positivism with theistic metaphysics.' (p. 194). These are valid criticisms - though the use of the term 'positivistic' is probably misleading because of its present connotations.

## 5.5.1 Sedgwick, Miller and Semi-Deism

To Gillespie Sedgwick is a 'nescient nomothetic creationist', to Hooykaas he is a 'semi-deist'.<sup>1</sup> Hooykaas distinguishes four basic positions:<sup>2</sup>

- (1) Atheism: 'the Necessity of immanent law rules over nature. No design, or plan, no final causes are admitted.'
- (2a) Deism: 'God created matter and endowed it with laws from which the world and all its inhabitants ensued according to the plan originally contrived in order to be realised in the future.'  
(This may either be seen as contrived mechanically or teleologically).
- (2b) Semi-deism: 'nature runs its constant 'natural' course unless God intervenes in a supernatural way'.
- (3) The 'biblical view': 'God created the world and designed it according to his free will and not by following a rational or ideal pattern to which he felt himself bound. God sustains the world in its minutest details, so that nothing happens without his "intervention".' (He goes on to quote Newton).  
'In His providence, God usually guides the world according to constant rules, but, as He is a free agent, He gives order as well as deviation from order.'
- (4) The supranaturalistic view: 'God (or the gods) acts in a rather capricious way in this world.'

These categories have some useful distinctions, but it does seem strange that they are defined with no reference to attitude to redemption and revelation. However regularly or irregularly natural phenomena are, one would have thought that Deism and the 'biblical view' would have differed primarily in THE great act of intervention

in human history in Jesus Christ, and in giving the biblical revelation about him. Perhaps Hooykaas is not intending to make these particular issues the distinguishing marks of the positions involved, but merely stating the implications regarding the relationship of God to creation for each one. So if we take this limited field, what exactly is Deism? On p.179 he seems to ascribe to at least some Deists the idea that creation was bound to a plan of inner necessity. But he goes on to add: 'A less rigorous form of deism advances the doctrine of a creation by which permanent laws were bestowed upon nature; these laws continue in operation by a subsequent divine sustenance of the universe.' So where does this leave any distinction between this 'less rigorous form of deism' and the 'biblical view'? It is difficult to see how any 'biblical view' could not see e.g. in Genesis chapter one the idea that God created a world to behave in a law-like way. Any scientist, moreover, in asserting the possibility of science at all must believe that most of the time the operations in the physical world are law-like - and Hooykaas is presumably not denying that a scientist can have a 'biblical view'. So what is the distinction?

Having said that it: 'may appear odd to place a large number of orthodox Christians (Buckland, Sedgwick, Conybeare, and Murchison<sup>3</sup>) into this group of Deists, he says: 'The Shibboleth by which they may be recognised is "divine intervention in the course of nature".'<sup>4</sup> Catastrophists, according to Hooykaas, 'only grafted a "theistic" branch on the deistic tree' because they assumed that God's intervention in nature's regular course marked off natural from supernatural. He later, and in apparent contradiction, says that Buckland and Sedgwick held the catastrophes to be God's intervention but not supernatural.<sup>5</sup> Only Miller, he says,

'is a type that might hardly be reckoned semideistic', for 'his religion was essentially based on other grounds than those afforded by the study of nature'<sup>6</sup>, and only Miller did not see natural theology as a scheme to which every reasonable man should consent, and which should be crowned by special revelation, accepted through divine grace.<sup>7</sup>

This distinction between Miller and Sedgwick is even stronger in C.C. Gillispie's Genesis and Geology. Miller 'required a divinity rather than a landscape gardener for his God, and whose Christianity centred around the redemption, salvation, and immortality of the individual soul.'<sup>8</sup> Hugh Miller 'was the only one whose conception of divinity contained many elements of spirituality'.<sup>9</sup>

The evidence surely does not bear out these statements. Sedgwick is just as concerned as Miller with redemption and salvation of the individual; his conception of God is spiritual, and (at least in some places) he shares Miller's Baconian emphasis on the independence of science and theology. Miller said he 'never tired' of Bacon<sup>10</sup>, and Sedgwick quoted him many times. The only definitely pre-revelational role of natural theology in Sedgwick is in his assertion that our experiences must enable us to form a conception of God otherwise revelation would have no meaning to us. Unlike some Deists, he says: 'in reasoning of creation, we dare not, we repeat, tell beforehand of what God must have done. This is rashly and irreverently to anthropomorphise God.'<sup>11</sup> and this was his ground of disagreement with both Whewell and Brewster in the plurality of worlds debate.<sup>12</sup> Sedgwick's Baconian induction was linked to his piety - both the book of God's works and the book of God's words had to be read humbly and inductively.

As for the 'shibboleth', Sedgwick seems generally to prefer the word 'interposition' rather than intervention, and Miller uses the

phrase 'direct interference on the part of the Deity in the work of creation' in one place.<sup>13</sup> Now one may agree with Hooykaas that there is a conceptual difference between an external but occasionally 'interfering' Deity (like Zeus), and an immanent Deity to whom a 'supernatural miracle' is simply a variation in this pattern of working. But a search for 'shibboleths' is not a reliable way of telling who believed which. To illustrate this, one might note that Cannon in his article on miracles<sup>14</sup> paraphrases Buckland's words about God 'continuing to superintend, direct, modify, and control the operations ....' to speak of 'God the Interferer'. But, strictly speaking, to 'modify' one's pattern of working is not the same as to 'interfere' with something, and this is not, in the context, the emphasis Cannon himself was making. The verbal change is not significant.

#### 5.5.2 Sedgwick, Miller and Kingsley

Objection has been made to any suggested distinction between the supposed 'semi-deism' of men like Sedgwick, and a 'biblical view'. The objection is not lessened towards the slightly altered position Hooykaas adopts in his part of the 1974 Open University book: New Interactions between Theology and Natural Science. In this he retains the basic framework of the five views cited at the start of this chapter, but now classes Miller and Sedgwick both as semi-deists. We are not now told explicitly a name of anyone who holds the 'biblical view', though it might be inferred that Charles Kingsley 'on the orthodox side' does<sup>15</sup>. Kingsley, admittedly, is less like Sedgwick than Miller is in theology, but the overall classification is no less unsatisfactory. Hooykaas defines 'the biblical position' thus:

'God created the world as an expression of His free will, and he sustains it in its minutest detail, so that nothing happens

without his 'intervention' in this sense. In His providence, He usually guides the world according to constant rule; but as He is a free agent, He may give deviations from order, as well as order.'<sup>16</sup>

There is nothing in this with which Buckland, Sedgwick, or Miller would have disagreed. Buckland's words in 1820, already quoted above,<sup>17</sup> specifically recognised that the term 'interference' was only a manner of speaking, for really all cause is a product of God's continuing will and exertion. Buckland is not asserting in any sense a kind of independence of natural law, but simply extending the 'traditional proofs from miracles and prophecy (i.e. from things not usually seen under 'natural law') accepted both by Paley and (as we have seen) by orthodox Evangelicals. Ironically, it would be Coleridge in this period who disliked talk of miracles as 'suspension of natural law' and emphasized instead their sign value.<sup>18</sup> Perhaps this is why Hooykaas chooses Kingsley, who stands partly in the Coleridge tradition, for his 'biblical view'. But it would be irony indeed to claim that Coleridge rather than (say) Wesley or Simeon represented the 'biblical view'. But, on this, any differences between (say) Sedgwick and Kingsley are apparent rather than real. We have quoted Sedgwick: 'The movements of his Will are the beginnings of every material cause...' Kingsley speaks of: what 'we call the Laws of Nature, though really they are no Laws of Nature, but merely customs of God; which he can alter as and when He will.'<sup>19</sup> Sedgwick's language differs, for he is usually dealing not only with the truth, but with epistemology. He is concerned with the way in which we come to know and recognise the agency of God in our world. Thus he says:

'The kingdoms of nature are presented to our senses in a succession of material actions ... All these changes and movements among the things around us seem to be produced by powers of nature we call second causes: but the mind of man cannot and

will not rest content with second causes, and is constrained to look above them to some First Cause ... we are naturally led to a conception of an intelligent First Cause capable of producing all the phenomena of the visible world.'<sup>20</sup>

When we speak of 'second causes' then 'our language defines correctly the manner in which the phenomena of nature are reflected in the human mind'. But then we ask how they began and so 'are we led to speak of the creative power, as well as of the sustaining power of God.' Both, to Sedgwick, are equally dependent on the continuing free will of God.

Sedgwick thinks it an 'utter confusion of thought' to confuse creative with sustaining power, though elsewhere he emphasizes that the differences between the two in the mind of God will always remain beyond our comprehension.<sup>21</sup> But there is a real difference as far as our human knowledge is concerned - and this is what Sedgwick is concerned with. Kingsley, in the passage cited by Hooykaas<sup>22</sup> is emphasizing his own feeling of awe in scientific exploration, and the Godward view of miracles. But how far did he really differ from the so called semi-deists? Interestingly, the cited passage begins with a reference to Buckland:

'... the part of your letter which deserves a long answer, longer than I can give, is what you say about natural science and Dean Buckland. It is exceedingly comfortable to me, who knew nothing of him, as proving him to have been the wise man I believed him. ... my doctrine has been for years ... that below all natural phenomena we come to a transcendental - in plain English, a miraculous ground. ... This belief was first forced upon me by investigating the generation of certain polypes of a very low order. I found absolute Divine miracle at the

bottom of all; and no cause save that of a supremely imaginative (if I may so speak) as well as Almighty mind, carrying out its own ideas; but gravitation, or the simplest law, will show the same truth.'

Buckland and Sedgwick used language different from Kingsley, but all three were at base saying that only God's continual and active sustaining power could explain the existence of secondary laws. Sedgwick too knows that when he refers to 'what we call God's laws' he speaks from our point of view - the whole of science being directed to the discovery of those laws.

Sedgwick, believing in the scientific falsity or improbability of evolution, believed also that through geology one could prove God active in a manner different from the argument from the existence of law. Kingsley, in contrast, was more reluctant to emphasize such 'gaps' in knowledge, from fears of what might happen if they were later filled in. Sedgwick believed it impossible to refer 'all changes organic and inorganic, to a succession of constant material actions, continued during an eternity of past time.'<sup>23</sup> But he continued: 'Were this true [my italics] it would not touch our argument: and every instance of organic contrivance or material adaptation would be a phenomenon unexplained, except upon the supposition of a contriver.' Kingsley, even in 1871, could say:

'... if it be said that the doctrine of evolution, by doing away with the theory of creation, does away with that of final causes - let us answer boldly, Not in the least. We might accept what Mr. Darwin and Professor Huxley have written on physical science, and yet preserve our natural theology on exactly the same basis as that on which Butler and Paley left it. That we should have to develop it I do not deny.'



That we should have to relinquish it I do...' <sup>24</sup>

Kingsley, unlike Sedgwick, thought evolution plausible - but both men denied that it destroyed natural causes in itself. Both men, moreover, were dualists. Thus Kingsley believed that 'souls secrete their bodies as snails do shells' <sup>25</sup>, and would have objected no less than Sedgwick to a confusion of moral and physical.

No one, of course, would pretend that the views of Sedgwick and Kingsley were identical. Sedgwick, realising that materialism needed evolution even if evolution did not strictly speaking imply materialism, attacked materialism through attacking evolution. Kingsley, loathing materialism no less, feared to make a battle ground over evolution lest a failure by the anti-materialists in this area might seem to give the materialists an overall victory. In 1863 he writes: 'Darwin is conquering everywhere, and rushing in like a flood, by the mere force of truth and fact... But they find that now they have got rid of an interfering God - a master magician, as I call it - they have to choose between the absolute empire of accident and a living immanent, ever-working God ...' <sup>26</sup> The 'master magician' is, of course, a gross parody of the position of men like Sedgwick - but Kingsley's anxiety is that no one mistake a victory over the 'interfering' God for a solution of the real question. Therefore, he says in this passage, he is working out natural theology in the light of Darwin and Huxley. Later indeed, in 1871, he is more explicit. He fears not so much pantheism as positivism. <sup>27</sup> But fighting evolution had long since ceased to him to be a practicable way to combat it.

Is there, then, any sense in which either Kingsley or Miller could be said to follow a 'Biblical view', and Sedgwick not to do

so? The writers of the Bible, of course, distinguished the usual from the unusual. The first chapters of Genesis moreover, presented a God who worked according to laws: 'let each bring forth after its kind.' But this did not result in so strong a concept of 'natural law' as to lead to a distinction between 'natural' and 'supernatural'. In any case, it would probably have struck them as irrelevant. But a Christian who is a scientist of any kind must presume that God is not capricious but has customs or habits which are sufficiently regular for us to construct 'natural laws' to describe them. This must be as true for a Newton, a Miller or a Kingsley as for a Sedgwick. Some kind of distinction between 'natural' and 'supernatural' (whatever terms are used) is then unavoidable. Unless one wishes to deny the occurrence of 'supernatural' miracles altogether, this in turn means that (whatever it implies in the mind of God) to our eyes we may distinguish between 'miraculous' acts in which God alters His usual pattern of working (which we call natural law), and acts in which he does not. Since neither Sedgwick, Miller nor Kingsley would have wished to make any such denial, this distinction must be implicit or explicit in the theological systems of all of them, as it certainly would for any Evangelical. The care with which words are chosen to present this is more indicative of personality and literary style than theology.

5.5 Notes

1. Hooykaas, The Principle of Uniformity in Geology, Biology and Theology, p. 192.
2. Ibid., pp. 170-171.
3. It may also be odd to place Murchison, who was a theist but by his own admission not really a Christian believer, alongside Sedgwick. His religious views, given in Geikie's Life of Murchison, p. 263, are in letters printed in full in C.R. Craig, 'Letters Concerning the Cambrian-Silurian Controversy of 1852', Jour. Geol. Soc., 1971, 127, 483-500.
4. Hooykaas (ref. 1), p. 192.
5. Ibid., p. 200.
6. Ibid., p. 203.
7. Ibid., p. 223.
8. Gillispie, Genesis and Geology, p. 177.
9. Ibid., p. 180.
10. P. Bayne, The Life and Letters of Hugh Miller, 1, p. 305.
11. Sedgwick, 'Review of Vestiges', Edinburgh Review, July 1845.
12. See chapter 4.3.
13. Miller, Footprints of the Creator, p. 12.
14. Cannon, 'The Problem of Miracles in the 1830's', Victorian Studies, 1960-1, 4, p. 6.
15. Hooykaas (ref. 1), p. 70.
16. Ibid., p. 65.
17. Buckland, Vindiciae Geologicae, p. 18, (see above p. 187).
18. Barth states: 'Coleridge objects, always and everywhere, to

the traditional definition of miracle: "Suspension of the laws of nature! suspension - laws - nature! Bless me! a chapter would be required for the explanations of each several word of this definition." (Coleridge and Christian Doctrine, p. 39; this is quoting Coleridge's Literary Remains.)

19. Charles Kingsley, Letters and Memories of His Life, p. 221.
20. 5th Edition Discourse, p. xi.
21. See above, cited from 5th Edition Discourse, pp. ccxxxviii-ccxi.
22. From the Open University Unit, New Interactions between Theology and Natural Science, p. 72. Hooykaas is citing the material of Charles Kingsley, Letters and Memories of His Life, p. 220, though he gives no reference.
23. 1st Edition Discourse, p. 21.
24. Kingsley (ref. 19), p. 310.
25. Ibid., p. 254.
26. Ibid., p. 253.
27. Ibid., p. 311.

## 5.6 Sedgwick and Evolution

### 5.6.1 Background and the 'Vestiges'

The eighteenth century legacy had not only left the evolutionary ideas of Buffon, but Linnaeus had stated his final view in 1779 in his Amoenitates Academica 'that species are the work of time.' In the early nineteenth century, however, the only actual schemes on offer to the scientific world to do this were those of Lamarck and St Hilaire (excluding that of Erasmus Darwin which was more a poetic vision). In English science both were regarded as rather old fashioned and mystical, although in certain specific areas their contributions might be recognised. Cuvier, amongst their contemporaries, was seen as a more actualistic and positivistic scientist - which indeed in many ways he was. There was an increasing recognition of the intricacy of mutual dependence and balance of organs, making species transmutation seem unlikely. The geological record showed sudden arrivals of new species. Thus, whilst Sedgwick in his Discourse in 1833, recognised that 'transmutation of species' could hypothetically explain new arrivals, he called it 'no better than a phrensied dream'. In this nearly all naturalists of repute at that time would have agreed with him.

Into this relative harmony there dropped in 1844 an anonymous publication (later found to be by Robert Chambers) The Vestiges of the Natural History of Creation. It sold 20,000 copies, being amended in subsequent editions (three came in quick succession) to remove some of the worse naivities. From 1845 onwards Chambers appended the Explanations (largely stimulated by Sedgwick's 1845

review) which was scientifically much more sophisticated than the original. Its philosophical framework was stated: 'The Eternal One has arranged for everything beforehand and trusted all to the operation of the laws of his appointment, himself being present in all things.'<sup>1</sup> Chambers, in fact, said:

'It has been one of the most agreeable tasks of modern science to trace the wonderfully exact adaptations of the organisations of animals to the physical circumstances amidst which they are destined to live ... design presided in the creation of the whole - design again implying a designer, another word for a CREATOR.'<sup>2</sup>

He then says that he needs not repeat all this since: 'The Natural Theology of Paley, and the Bridgewater Treatises, place the subject in so clear a light ....'

However, in line with his general approach, organic evolution is introduced after an explanation (based on the nebular hypothesis) of the earth's origins:

'during the deposition of all the rocks, a gradual change of physical conditions was going on ... such variations in the ancient seas might be amongst the causes of that constant chance of genera and species in the inhabitants of those seas to which the organic contents of the rocks bear witness.'<sup>3</sup>

Chambers introduced the term 'missing link' and made some suggestions<sup>4</sup>.

He noted the jumps in fossil species, but explained them away (as Darwin did later) by imperfections in the sequence.<sup>5</sup> He was

actualistic but without making uniformitarian assumptions.<sup>6</sup> He

denied vitalism<sup>7</sup> and saw form as arising naturally in matter -

for which crystallisation is given as an example.<sup>8</sup> In fact, one of

the major faults in the book is its reliance on superficial resemblances,

as critics showed. Electrical forces were given great generative powers.<sup>9</sup> No real mechanism was given, and naive identifications were made, e.g.: 'it was announced some years ago by a French physiologist, that globules could be produced in albumen by electricity. If, therefore, these globules be identical with the cells which are not held to be reproductive ...'<sup>10</sup> Various old wives tales were presented about spontaneous generation. He suggested a version of embryonic recapitulation,<sup>11</sup> with length of gestation as some kind of altering mechanism.

#### 5.6.2 Reactions to the Vestiges - Miller and Sedgwick

The Vestiges contained obvious absurdities (e.g. dogs inheriting skill at dominoes<sup>12</sup>, rooks rearing orphans because of reason<sup>13</sup>, and rats hatching out from goose eggs<sup>14</sup>). Scientifically it was shallow, though Chambers' sequel Explanations was less so. It attracted, however, much attention, and many leading scientists reviewed it, with varying degrees of hostility.<sup>15</sup> Huxley, for example, was furious with it because he regarded it as an aberration of true science.<sup>16</sup> Owen's rather more ambivalent review has aroused an amount of speculation on his real attitude.<sup>17</sup> The two critics, however, at whose comments we shall particularly look are Miller and Sedgwick, and these have been chosen because Sedgwick's review has been unfavourably compared with Miller's and may be generally misunderstood. We will not, of course, be particularly interested in the scientific absurdities of the Vestiges (of which all critics pointed out many) but in the broader issues.

Miller asks:

'Has Nature, during the vast geologic periods, been pregnant, in like manner, with the human race? and is the species, like the individual, an effect of progressive development,

induced and regulated by law? The asserters of the revived hypothesis of Maillet and Lamarck reply in the affirmative. Nor, be it remarked, is there positive atheism involved in the belief. God might as certainly have originated the species by a law of development, as he maintains it by a law of development- the existence of a First Great Cause is as perfectly compatible with the one scheme as with the other; and it may be necessary thus broadly to state the fact, not only in justice to the Lamarckians, but also fairly to warn their non-geological opponents, that in this context the old anti-atheistic arguments, whether founded on the evidence of design, or on the preliminary doctrine of final causes, cannot be brought to bear.<sup>18</sup>

What Miller sees as the real problem is:

'If, during a period so vast as to be scarce expressible by figures, the creatures now human have been rising, by almost infinitesimals..., until they have at length become the men and women we see around us....'

Then either we must conclude that 'all the vitalities' of lower creatures are immortal, or that 'human souls are not so.'<sup>19</sup>

He also rules out the suggestion that God might have intervened directly to breathe immortality into the first human (though Lyell later seems to have favoured this) because he assumes that such intervention is expressly renounced by authors of such schemes. This then is his objection and: 'If I be in error at all, it is an error into which I find not a few of the first men of the age, -represented, as a class, by our Professor Sedgwick and Sir David Brewsters, - have also fallen.'<sup>20</sup>

Now contrary to what recent commentators have said about Sedgwick, his point of departure is exactly the same as Miller's, as he himself



claimed in a letter to Miller in 1849.<sup>21</sup> The main differences are that Sedgwick's is a more philosophically sophisticated critique, whilst Miller's is written in a very much clearer style.

To begin with we must remember Sedgwick's views (given in chapter 5.4 above) of God's relationship to the world. We might also note some further words in the 1st Edition Discourse in 1833:

'If, then our planetary system was gradually evolved from a primeval condition of matter, we may well believe, that every material change within it, from first to last, had been but a manifestation of the Godhead - Or we may suppose, that new powers have, by an act of creative interference, been impressed on it at successive epochs of its changes- and that these new powers, working together with the old, may have brought about the next system of material conditions. [Here Sedgwick inserts a footnote comparing this to the repeated changes in organic species which 'can only' be accounted for in this kind of way]. Or, if it be thought more in conformity with what we see of the modes of material action, to suppose that the primeval system contained within itself the elements of every subsequent change, then is the primeval matter to the natural system of the world as the seed to the plant, or the egg to the living creature ... [in this event he says] far from ridding ourselves, by our hypothesis, of the necessity of an intelligent first cause, we give that necessity a new concentration, by making every material power, manifested since the creation of matter, to have emanated from God's bosom by a single act of omnipotent prescience.'<sup>22</sup>

Why, we might ask, should God not have done something similar with

organic creation? Granted that Sedgwick (quite rightly) saw that on a purely inductive basis one would conclude that evolution was scientifically improbable, why should it have been also theologically unacceptable? The classic interpretation of Sedgwick (as we shall see) does not answer this question. The correct answer is really that it was not. But we need first to follow through carefully Sedgwick's review of the Vestiges (the first he wrote) in the July 1845 Edinburgh Review.<sup>23</sup>

The review begins (p.1-2) with a one page outline of the thesis of Vestiges, and its general structure and appeal. Then<sup>24</sup> Sedgwick gives his own brief view of the work: 'everything is touched upon, while nothing is firmly grasped ...'. The author does not understand scientific principles, and builds impossible hypotheses like castles in the air. So how then, he asks, can we account for its popularity? The general population are unable, as can scientists, to discern the shallow nature of Vestiges, but Sedgwick affirms that he knows no reputable scientist who accepts it. But the unsuspecting public:

'must therefore be plainly told that the philosophy of the author is borrowed from a false and shallow School- and that the consequences he dares to draw from it, so far as they are new in the scientific literature of our country, are nothing better than mischievous and antisocial nonsense.'<sup>25</sup>

Recent commentators, seemingly unable to see in all this anything but the doctrine of transmutation of species, seem to take Sedgwick to be talking of the philosophy of transmutation. But this surely is mistaken. He is referring to the wider issues of materialism, the identification of mind and consciousness with material elements,

and the reduction of morality and human responsibility. This will become clearer as we progress. There then follows a particularly obscure and (for today's taste) unfortunate passage, full of fulminations about 'glorious maidens and matrons' and good taste; about the quaintest and most 'Victorian' part of the whole Review. Perhaps it is this which has so influenced commentators (though it was, after all, 1845 ). But towards the end of the paragraph he alters tempo to get into the real point of departure of his objections to Vestiges. He laments that those glorious maidens and matrons he mentioned will read in Vestiges:

'that their Bible is a fable when it teaches them that they were made in the image of God- that they are the children of apes and the breeders of monsters - that he has annulled all distinction between physical and moral - and that all the phenomena of the universe, dead and living, are to be put before the mind in a new jargon and as the progression and development of a rank, unbending and degrading materialism.'<sup>26</sup>

What is important here is that this particular aspect comes at the end of Chambers' book - but Sedgwick picks it up first. It is the particular position of man, and the mind-brain problem which he begins with. This is significant, for it indicates that Sedgwick's basic objection was not so much the organic evolution as such, but that, like Miller, he thought Chambers' work contradicted the Christian view of man as uniquely in God's image.<sup>27</sup>

What exactly, then, was the theory of Chambers on issues of mind and morality which Sedgwick found so objectionable ? Chambers writes:

'Cannot the first cause of all we see and know have wrought matter itself from its very beginning, with all the attributes

necessary to develop into mind ...?'<sup>28</sup>

'The statistical regularity in moral affairs fully establishes their being under the presidency of law. Man is now seen to be an enigma only as an individual; in the mass he is a mathematical problem. It is hardly necessary to say, much less to argue, that mental action, being proved to be under law, passes at once into the category of natural things. Its old metaphysical character vanishes in a moment, and the distinction usually taken between physical and moral is annulled. This view agrees with what all observation teaches, that mental phenomena flow directly from the brain. They are seen to be dependent on naturally constituted and naturally conditioned organs, and thus obedient, like all other organic phenomena, to law.'<sup>29</sup>

By this Chambers explains consciousness, going on to argue for the 'absolute identity of the brain with a galvanic battery' - though he does add that electricity is 'almost as metaphysical as mind was ever supposed to be.' Chambers states: 'The difference between mind in the lower animals and in man is a difference in degree only - it is not a specific difference.'<sup>30</sup> Thus: 'Conception and imagination appear to be only intensities, so to speak, of the state of the brain in which memory is produced.'<sup>31</sup>

A most important corollary is that human moral responsibility is illusory. Criminals (Chambers concludes) are victims either of their social conditions or their inherited characteristics, and criminal jurisprudence should be addressed exclusively to reformation and not to punishment.<sup>32</sup> The question as to whether morality has any meaning at all without responsibility is not faced.

Sedgwick's point of departure and passionate renunciation of all this has already been given above. His fundamental objection to Vestiges is that it confuses mind with brain, consciousness and will with material causes. So he comments:

"Electricity is almost as metaphysical as ever mind was supposed to be"...and yet electricity is a real thing, and actual existence," or in other words, a material existence. In the same passage he tells us "that the brain is absolutely identical with a galvanic battery!" As well might he say that the human will and the point of a needle are identical, because each of them can produce the contraction of a muscle. Allowing that some of the functions of the brain resemble galvanism, are we to conclude that all its functions are galvanic?' <sup>33</sup>

Sedgwick's point here is basic to his rejection of Vestiges. To Sedgwick, as to many others, mind is not the same as brain.

'What we call mind is that principle which binds our thought together, and makes us intellectually what we are - giving us a unity of consciousness not transferable to another or separable into parts - a unity of knowledge, a unity of responsibility, and a unity of aspiration after future good.' <sup>34</sup>

Sedgwick was a kind of dualist, he saw 'mind' and 'matter' as different kinds of thing. So he says:

'the mind is immaterial, though mysteriously connected with matter and its laws, (should any one affirm that they must for ever remain connected, we have no dispute with him, for the subject is far above our knowledge) ...' <sup>35</sup>

Man has been 'driven to invent a new language, the symbol of pure abstraction, the fit instrument of reason', moreover 'there is an immeasurable difference between instinct and reason'<sup>36</sup>. The mind of man, then is different in kind from that of animals according to Sedgwick, in the kind of its consciousness. And consciousness is not material. On its connections with matter he expresses what might be termed agnosticism or nescience. He is a cautious and sophisticated dualist, and his dualism is basically consistent.<sup>37</sup>

His dualism apparently entered his lectures, for in the Sedgwick collection at Cambridge University there is a set of notes taken by a Mrs. Tovey speaking of a great gulf between man and beast:

'A man can form an abstract notion, give it a name, and make it a material for abstract reasoning ... It is that gift of the Almighty that makes the gap between man and the brute creation.'

He described the skull as:

'containing the mechanism which elaborates thought, but which is no more thought itself than the pipes of the family organ are the music of Handel.'<sup>38</sup>

This does not, of course mean that he is defensible on all details, nor that he is always consistent in working out implications of his position. His style, moreover, was sometimes unclear - he was writing in passion and anger. He was, however, no less certain than Miller of why he objected to the Vestiges, and his overall system was more philosophically sophisticated than Miller's. That Miller's style was better is hardly surprising for Miller was an acknowledged master of prose. Sedgwick, in anger, often exaggerated - as he did in other controversies such as the Beverley affair and the controversy with Dr. French,<sup>39</sup>

Now by page 17 of his review Sedgwick has made his preliminary fulminations against the Vestiges, and he has outlined (p.5-16) his reason for objecting to the whole conception and classing it as 'rank materialism'. He then proceeds to a more orderly critique, more in the order of the Vestiges itself. The Nebular Hypothesis has been taken by Chambers as fact. To Sedgwick it is a 'splendid vision' which may or may not turn out to be true. This is interesting. Sedgwick uses the over-dogmatism of Chambers((which incidentally was toned down in the 5th Edition of Vestiges) to attack him. But it is not because he regards the Nebular Hypothesis as of itself any theological significance. It is because any weapon will be useful to discredit the Vestiges. So Sedgwick calls it 'the raving madness of hypothetical extravagance'.<sup>40</sup>

When he comes next to the refutation of the transmutation of species as accepted by Chambers, it seems to be in similar spirit. From p.29-49 Sedgwick is mostly simply outlining geological history, but on p.31 he quotes:

"The first step," he tells us, in the creation of life upon this planet was "a chemico-electric operation by which simple germinal vesicles were produced." The next step was "an advance, under favour of particular conditions, from the simplest forms of being to the next more complicated, and this through the medium of the ordinary process of generation." All this is confirmed by an appeal to Mr Babbage's calculating machine, and by a geometrical figure- and our author adds, very pleasantly and with a logic, we hope, peculiar to himself, "though this knowledge were never to be clearly attained, it need not much affect the present argument; provided it can be shown that there

must be some such influence within the range of natural things!"

We reply, show this and we have done. We have nothing but bare assertion; and we defy him, and all the materialists on the face of the earth, to prove this single point ...'

It is important to note the context of this. Sedgwick does not say that because Chambers believes in transmutation of species he must be classed as a materialist. Sedgwick has already shown that Chambers is a materialist from his comments on mind. Sedgwick's assumption is (rightly) that transmutation is a necessary (though not sufficient) tenet for materialism. To the materialist the hypothesis is essential, and in the fact that all geological observation is against it Sedgwick finds a weapon to attack the materialists.

On p.50 Sedgwick returns from details of geology to broader issues. He reaffirms his actualism - and challenges Chambers to prove or show probable the development hypothesis from living nature. But over and again he emphasizes that transmutation is anti-inductive, based on circular reasoning not on observation, etc.

On p.62 Sedgwick turns to deal with Chambers' claim that a view of God as creating by fiat 'anthropomorphised God' and 'it is no fitting mode of creative intelligence that he should be constantly moving from one sphere to another.'<sup>41</sup> Sedgwick meets this on several levels. First, he totally rejects Chambers' caricature of the theist position as God the interferer: if the inductions of science do show us progressive creations:

'Does the conclusion at which we have arrived degrade our notion of the Godhead and of his creative power? We think far otherwise. The law of creation is the law of the Divine Will, and



nothing else besides- and, as the children of nature, how are we to know that will, except by honestly reading the book of nature? The fiat of the Almighty was sufficient at all times, and for all the phenomena of the universe - material and moral. It may be true, that in the conception of the Divine mind there is no difference between the creation of dead matter and its unbending laws, and the creation of organic structures subservient to all the functions of individual life. But such views are, and must be, above our comprehension, and only lead us from the right way of ascending, step by step to the conception of natural laws, governing the kingdom of nature, organic and inorganic. Each organic structure is a miracle as incomprehensible as the creation of a planetary system... What know we of the God of Nature (we speak only of natural means) except through the faculties he has given us... [Sedgwick then continues as already quoted above on p. 188]...<sup>42</sup>

All force is will force. To a genuine Christian theist it is certainly not degrading to imagine God sometimes varying his patterns of working, in 'miracles' or 'fiat creation'. And Sedgwick reacts in horror to Chambers' suggestion that such a view implies 'blemishes and blunders' in creation. God is not 'interfering' to patch up mistakes, but acting as part of an overall creative strategy.

As for anthropomorphising:

'we can have no help for it. We have no conception of God, nor can we ever have, except through such faculties as he has given us. Humanize his attributes we therefore must, or express ourselves in mere negations.'<sup>43</sup>

Here Sedgwick is defending the theistic view against attack. He therefore points out both the grandeur of its concepts and the

problems of the alternative system which can easily lead to atheism or pantheism. Finally, he objects to the Deistic tendency in Chambers to 'go at once to the great First Cause and tell our readers what he MUST have done'<sup>44</sup>. A proper inductive humility is to study God's works and his word and find out what he has in fact done.

The interpretation which has here been suggested is borne out by the way in which Sedgwick returns to the issues in the preface and appendices to the 5th Edition Discourse. After a few paragraphs introduction, Sedgwick says that all of us see in nature around us harmony and order. But 'the mind of man cannot and will not rest content with second causes, and is constrained to look above them to some First Cause'<sup>45</sup>. He contrasts this with the school of St Hilaire, who deny the possibility of ascending to general laws of nature. Sedgwick's basic argument is 'contrivance proves design'. His discussion of this, and of dangers seen by Bacon in the misapplication of final causes lasts until page xvii. He then has a new heading: 'Theory of Spontaneous Generation, Transmutation of Species, etc.' He suggests that those who introduced the theories wished to escape the conclusion that there was a Designer. After briefly describing the theories of spontaneous generation and transmutation he continues:

'The authors and early defenders of this theory were, perhaps without exception, unbelievers in every form of Revealed Truth. They were materialists in the rankest sense of that term. They denied all distinction between material and moral phenomena - regarding them both as nothing more than the varied manifestations of the powers of second causes. Most of them formally denied all proofs of design in nature, and in all indications of an overruling Providence- and thus struck at the foundation of

Natural Religion. But a doctrine may be true and yet may be turned to evil purposes. The first questions for discussion are the following - Is the doctrine true? Has the animal kingdom been first produced by spontaneous generation and afterwards perfected by transmutation and progressive development?...<sup>'46</sup>

We note Sedgwick's objections. He places the school's rejection of revealed religion before any comment on natural religion. He objects secondly to their 'rank materialism'. This, surely, is not merely that they believed in transmutation, but that they denied the reality of the spiritual. Then he objects to their regarding moral phenomena as epiphenomenal to matter. ONLY THEN, does he speak of their attitude to the argument from design. Moreover, he goes on specifically to say that the transmutation doctrine could still be true though misused by Chambers and his school. In his view, though, there is not much point in discussing this if the doctrine itself is demonstrably false anyway. Since, as we know, he thought it was, he chooses to consider this first. The hundreds of pages of argument against the Vestiges with which the 5th Edition Discourse is packed, should not obscure the basic point of departure. It is the underlying materialism of the school out of which the idea arose, rather than the idea itself, which he finds objectionable. Natural religion is just one of the things this school deny - and that not, by any means, the first which Sedgwick chooses to mention.

### 5.6.3 Modern Scholars, Sedgwick, and the Vestiges

A number of modern commentators have analysed Sedgwick's attitude to Chambers, in varying degrees of depth. One of the first was C.C. Gillispie (in Genesis and Geology), whose analysis

makes several basic assumptions which are questionable or invalid. First (as we saw above on p. 224) Gillispie makes a sharp distinction between Sedgwick and Miller. This distinction is also applied to their respective reactions to the Vestiges. Sedgwick's 'rather incoherent opposition to the idea of development' is contrasted with that of Miller who:

'...knew exactly why he stood where he did...[he] derived his opposition to development from values older and sounder than any Sedgwick felt, expressed, or understood.'<sup>47</sup>

Further, Sedgwick's whole approach is seen as materialistic:

'For Sedgwick and his school... truth had first to be found in materials apprehended by sense, and then it had to be "ennobled" into morality. One might think that this "annulled the distinction between material and moral" as effectually as Chambers had, but not so.

Does the conclusion at which we have arrived degrade our notion of the Godhead and of his creative power? We think far otherwise... [Gillispie here quotes the words of Sedgwick already given above on pp. 241-2] ...

In order to invest their materialism with moral quality, the scientist theologians for whom Sedgwick spoke called in what was essentially a series of miraculous dispensations to account for the ultimate dispositions of the materials which were their most certain reality... The trouble really lay in the difficulty of relying primarily on the material evidence for the existence and continuing activity of the Deity without accepting materialism - a dilemma which pious and good-hearted men of limited imagination could dodge only by denying self-sufficiency to the cosmic order...'<sup>48</sup>

The very essence of physico-theology, of course, lies in arguing from the physical to characteristics of a Designer, i.e. to spiritual truth. But this need not imply that those who accept it (whether Sedgwick, Miller, or any other Evangelical) see it as the only or primary way to such knowledge. Gillispie, in quoting Sedgwick, omits an important parenthesis: 'What know we of the God of nature (we speak only of natural means)...'<sup>49</sup> Sedgwick is answering the taunt that his physico-theology would imply an inferior kind of God - he is not implying that it is the primary means to know of such a God.

A second point is that the essence of materialism (as Sedgwick understood it) was a denial of a difference in kind between moral and material. Chambers had done exactly that by arguing that since mental action was under law it was of the category of natural things, and the distinction between physical and moral thus 'annulled'. But physico-theology does not imply mind to be of the same order as the physical. The jump of inference is from our own internally experienced volition causing rearrangement of material things in design, to the general existence of a Designer where design is manifested. But this nowhere annuls the very basic distinction between material and moral. Neither does it imply that all truth has 'first to be apprehended by sense', for the unity of consciousness is not so apprehended.

Since, however, Gillispie sees Sedgwick (though not Miller) as 'really' a materialist, he sees Sedgwick's 'hysterical'<sup>50</sup> outburst against the Vestiges as being because he was

'a scientist upon whom had suddenly flashed the full implications of his own endeavours, and who refused to understand them...'<sup>51</sup>

There are a number of objections to this kind of 'suppressed guilt' theory. Firstly, as we have seen, Sedgwick did not base his

Christian belief solely on physico-theology. Secondly, his objection was primarily to the philosophical materialism, which is not a direct implication of evolution. Thirdly, physico-theology itself does not depend on provable 'gaps' (though it may be strengthened by them). Finally, only with a great deal of hindsight can evolution be seen as a 'logical' implication of Sedgwick's geology, even on a purely scientific level. Sedgwick's 'hysteria' surely relates rather to his character, for he exhibited it in other controversies where no 'suppressed guilt' might be supposed.

A decade after Gillispie's book, Hooykaas published similar views on Sedgwick and Miller on this issue.<sup>51</sup> More recent analyses have tended to add elements of Gillispie's analysis to concepts of a liberal Anglican 'Cambridge Network'.<sup>53</sup>

Brooke, for example, explains the unpopularity of the Vestiges amongst scientists:

'partly... by the philosophical and scientific naivety of the book, partly by the fact that Chambers' theodicy cut straight across the argument from adaptation to design as it was understood by Sedgwick or Whewell. But it is perhaps more fully explained by the fact that Vestiges represented a reductio ad absurdum of the mediating role of appeals to design. By launching such appeals in the context of a development hypothesis that smacked of both materialism and determinism, the author of Vestiges had sold the pass. On this point the perception of R.W. Church is as revealing as the ire of Sedgwick,<sup>54</sup> or the judicious ambivalence of Herschel.'<sup>55</sup>

In chapter 6.4 we will consider whether there is any evidence that natural theology did or even could play a 'mediating role' in this way. But Brooke's article again concentrates on the

transmutation issue, and the damage to natural theology as the central issue. In fact, as we have seen, Chambers' theory not merely 'smacked of' materialism, but was blatantly materialistic - and it was this which was Sedgwick's more basic objection.

Thackray and Morrell seem affected by Brooke (repeating his phrase 'sold the pass') but are less careful:

'The Bucklands and Sedgwicks encouraged science as a secular pursuit, while claiming that it led savants not towards materialism and determinism but towards God. That was why Robert Chamber's Vestiges of Creation, published anonymously in October 1844, roused their fierce anger; by taking the logical step of associating design with materialism and determinism, Vestiges showed the vanity of proclaiming that science led naturally to religion; and it undermined the notion that science and religion were in ultimate diapason. If Vestiges sold the pass, the liberal Anglicans in the British Association had already smoothed the way. In the context of the Association their Christianity amounted to 'nothing more than a series of pious or polemical remarks upon the physical world viewed religiously'. There was indeed little doubt that gathered together in the British Association, the liberal Anglicans worshipped at the shrine of science rather than at that of the Christian God.'<sup>56</sup>

Whilst appreciating the wealth of researched material in this book, the religious analysis, as we have already seen, appears confused and misleading. We have already noted above the apparent classification of Chalmers as a 'liberal natural theologian'. This passage just quoted raises a number of further questions. To call Chambers' materialism the 'logical step' is indefensible even with hindsight.

What Sedgwick objected to was the confusion of spiritual/moral terms and physical ones. There are many people to whom some form of dualism is self evident, and Chambers' step is anything but logical. Secondly, the author of the Vestiges makes clear his own belief in a God and the validity of design arguments,<sup>57</sup> so the Vestiges cannot be said to show the 'vanity of proclaiming that science and religion were in ultimate diapason.' Sedgwick did not object because the author of Vestiges was irreligious, but because his materialism clashed with Sedgwick's Christian dualism. It is questionable how far organic evolution as such would have clashed with this, but the confusion of moral and physical did. It was, moreover, no surprise to Sedgwick to find that science could be linked with materialism in this way for he (like others in his group) were well aware that continental scientists had so linked it. Neither evolutionary Deism<sup>58</sup> nor scientific materialism were new, and it is difficult to see exactly what 'pass' Chambers was supposed to have 'sold'. Sedgwick nevertheless thought that materialist scientists were wrong, and that science led more naturally to a theistic dualism in which experience of the natural world pointed to a creator God, and the experience of conscience, consciousness and the moral, pointed to a moral God. The two categories cannot be equated (as Chambers tried to do) at the level of immediate experience, but are linked at the ultimate level as the two 'Gods' are recognised as identical. Evolution, as such, would not have in any way demolished this schema (though it might present some problems of interpretation for a Christian who went further and identified this God of natural theology with a God self-revealed in Scripture). The presence of an evolutionist as such in the BAAS was no more threatening than the presence of a Unitarian.



The penultimate sentence of the above quotation is in a sense true. A multi-denominational meeting of scientists to discuss scientific issues (i.e. issues about physical laws) is not a meeting for religious worship. Neither liberal Anglicans, Evangelicals, Tractarians, nor anyone else imagined that it was, and two things properly limited it to this. The first was that no one claimed that study of physical phenomena could lead further along the path of doctrine than the recognition of a creator God - with the additional fact (according to the Chalmers tradition) that he was moral in disposition. The implications of the physical study could not lead to the doctrine, for example, of atonement or Trinity - neither the apostle Paul in Romans 1, nor anyone else, imagined that it could. The BAAS limited themselves to what they (as, in the main, Christians) saw as the implications of their study. Secondly, whilst interdenominational meetings for business (e.g. in the Bible Society) were not unknown, those for worship were rarer.<sup>59</sup> To have held them at a scientific gathering would have been to court tremendous criticism.

But to phrase this as: 'their Christianity amounted to nothing more ...' is unfair. To an Evangelical like Chalmers (or Walker at Oxford), what he learned of God's physical creation he could assimilate in his mind as the work of the God he knew to be the Trinity with Christ as his Redeemer. That others might not see it so was irrelevant.

Thackray and Morrell's final statement about 'worshipping at the shrine of science' is emotive and misleading. Neither Chalmers, Brewster, Walker, Simeon, Sedgwick, nor any other of Evangelical sympathy who attended the BAAS was 'worshipping at the shrine' of science. They all saw redemption rather than knowledge of physical laws as ultimate, and most (including Sedgwick) explicitly said so. T.H. Huxley could well be argued from the way he speaks

of science in his letters and writings, to have substituted it for God, but there are many things which although they are not inherently idolatrous can be made so. Evangelicals valued science, but neither worshipped science nor set the God of natural theology in place of the God of the Bible.

It would seem, finally, that if my interpretation of Sedgwick is correct, then it has a bearing on the issues recently raised by Robert Young, when he comments on the apparent preoccupation in the period with geology in discussing man's place in nature.<sup>60</sup> People like Sedgwick (and one could parallel e.g. Brougham's discussion of mind, brain and materialism in his Discourse<sup>61</sup>) did show a concern for the mind-brain problem. But Sedgwick simply did not believe that understanding of the physical parts of the brain (e.g. on the phrenological system) would 'reach the philosophy of mind.'<sup>62</sup> It is at least a plausible hypothesis that the apparent preoccupation with geology was because people like Sedgwick thought that there they could on a clear empirical basis knock out a prop from the materialists - and the rather more cautious approach of Whewell (which Young records) could partly stem from being less certain than his colleague Sedgwick that the geological data was indeed against evolution. But this is hypothesis.

#### 5.6.4 Sedgwick and Darwin

Darwin published his Origin of Species in 1859, and Sedgwick reacted critically. In Darwin, of course, unlike in Vestiges, he recognised a respected scientist and personal friend. Darwin's work, unlike the Vestiges, is purely scientific. It does not pronounce on the nature of man, or the identification of morality with epiphenomenal effects of material development. Sedgwick's review of it in the Spectator<sup>63</sup> is also basically scientific. He objects (and quite

validly in view of the evidence then available) that it is non-inductive, and non-actualistic. In a private letter to Darwin he says that Darwin has deserted the inductive path and then:

'We all admit development as a fact of history; but how came it about? Here, in language, and still more in logic, we are point-blank at issue. There is a moral or metaphysical part of nature as well as a physical...'Tis the crown and glory of organic science that it does, through final cause, link material to moral; and yet does not allow us to mingle them in our first conception of laws .... You have ignored this link; and, if I do not mistake your meaning, you have done your best in one or two pregnant cases to break it....'

Sedgwick fears that Darwin is set to deny the independent reality of morality, and the fact that the universe is designed. This is what, surely, he means by 'linking material to moral' through final cause. Interestingly, Sedgwick goes on to say that

'If your development produced the successive modification of the bee and its cells (which no mortal can prove), final cause would stand good as the directing cause under which successive generations acted and gradually improved.'<sup>64</sup>

Final causes (i.e. design) need not be ruled out by transmutation - or even natural selection. But Sedgwick feared that Darwin was moving to a denial of these (which denial would, of course, be a metaphysical rather than a physical statement).<sup>65</sup> Sedgwick also seems sometimes to see Darwin's theory through the distorting medium of the philosophical position of the Vestiges. Thus he wrote to Miss Gerard:

'Darwin's book... is the system of the author of the Vestiges stripped of his ignorant absurdities, It repudiates all reasoning from final causes; and seems to shut the door upon

any view (however feeble) of the God of Nature as manifested in His works. From first to last it is a dish of rank materialism cleverly cooked and served up...<sup>66</sup>

This is not very fair to Darwin, though one wonders how far Sedgwick simplified the issues to someone who could perhaps not be expected to follow the philosophical niceties. In a letter around the same time to Owen, Sedgwick is more circumspect, arguing that Darwin's work is non-inductive, but saying only that it 'savours of rankest materialism'.<sup>67</sup> Put thus, Sedgwick is correct, for by then the theory of evolution itself had long been associated with (in Sedgwick's terms) materialism.

5.6 Notes

1. Chambers, Vestiges of the Natural History of Creation, p. 184.
2. Ibid., p. 326.
3. Ibid., p. 68.
4. Ibid., p. 88.
5. Ibid., p. 125.
6. Ibid., p. 147
7. Ibid., p. 170
8. Ibid., p. 166.
9. Ibid., pp. 173, 186, 205, etc.
10. Ibid., p. 173.
11. Ibid., p. 193.
12. Ibid., p. 340.
13. Ibid., p. 339
14. Ibid., p. 220.
15. See The Life and Letters of T.H.Huxley, 1, pp. 179-183.
16. See J.H. Brooke, 'Richard Owen, William Whewell and the Vestiges, BJHS, 1977, p. 132.
17. See e.g. Millhauser, Just Before Darwin, Brooke (ref. 16), and A. Desmond, Archtypes and Ancestors.
18. Footprints of the Creator, p. 12.
19. Ibid., p. 14.
20. Ibid., p.16.
21. Clark & Hughes, 2, p. 161.

22. 1st Edition Discourse, pp. 24-5.
23. The Edinburgh Review, 1845, vol. lxxxii, number cvxv, 1-85.
24. Ibid., p. 2.
25. Ibid., p. 3.
26. Ibid.
27. The Macvey Napier papers in the British Library (Vol. xv, Pressmark ADD 34625) contain a number of letters of Sedgwick in 1845 just before his review of the Vestiges, and these bear this out. On January 27th he wrote to Hopton: 'I have seen the work you mention, but have not had time to study it... But the doctrines of a gradual transmutation of species I utterly abominate; and I only abominate it because I believe it to be utterly untrue.' Sedgwick was perhaps consulted because of his expertise relating to the transmutation question, but he makes it clear his basic ground of objection is one of fact rather than religion. Having read the Vestiges, on 10th April Sedgwick says he detests its scientific shallowness, its 'gross materialism', and its 'shameful shuffling of the facts of geology.' He expresses doubt whether the author 'knows the mischief of her own views. They are the favourites of the ultra materialist school of France. I need not tell you for you know far better than I can tell you, how shallow are her metaphysics. With her the bellowing of an ox... are phenomena of the same order with the abstractions of language - the creations of pure intellect ... Our author knows nothing of our moral nature & of conscience as distinguishing us from beasts - as giving rise to human law and human responsibility ... how does the author account for our future expectation and hope - the foundation of a religious nature; and again proved by moral

expression often suited to our moral condition.' It is not the damage to the natural theological arguments which upsets Sedgwick, but the materialistic view of man. At that time Sedgwick expressed loathing of the Vestiges, but declined the review task. By 17th April he had accepted it, and begins to map out his critique. On 26th May he expresses delight at the way the geological arguments against the Vestiges have turned out, for he has never before actually arranged them in such a way although all the facts were known to him. Certainly in these later letters Sedgwick does refer to the argument from design, as he does indeed in his final review. But the early letters make it clear that his point of departure giving rise to his moral indignation against the Vestiges is in its view of man - not in its damage to natural theology. Moreover his scientific sensibilities are genuinely outraged at its utterly unscientific, credulous, and sometimes downright silly nature.

28. Chambers (ref. 1), p. 329n.

29. Ibid., pp. 333-34.

30. Ibid., pp. 336-38.

31. Ibid., p. 345.

32. Ibid., p. 361.

33. Sedgwick (ref. 23), p. 5.

34. Ibid., p. 13.

35. Ibid., p. 16.

36. Ibid.

37. It may just be worth noting at this point that a mind-brain dualism is not necessarily to be regarded as a peculiarity of Sedgwick's

era, rendered obsolete by the rise of 'truly positivistic science'. In modern times the neurophysiologist Sir John Eccles, together with the philosopher of science Sir Karl Popper, has presented a mind-brain dualism e.g. in The Self and Its Brain, The Human Mystery, and The Human Psyche. An alternative form of mind-brain dualism has been adopted by D.M. Mackay, Professor of Communication at Keele University, in The Clockwork Image, Christianity in a Mechanistic Universe, Information, Mechanism and Meaning, and Human Science and Human Dignity. Sedgwick's dualism is consistent, even if we have to adopt a 'Whiggish' practice of assessing it by 'modern' ideas.

38. The notes are in Box 13 of the Sedgwick collection.
39. Clark & Hughes, 2, p. 237, etc.; 1, p. 410 etc.; Dr. French's heirs complained to Clark & Hughes (in a letter in the Sedgwick collection at Cambridge) that they had been too critical of Dr. French.
40. Sedgwick (ref. 23), p. 25, etc.
41. Ibid., p. 63.
42. Ibid., p. 62.
43. Ibid., p. 63.
44. Ibid., p. 67.
45. 5th Edition Discourse, p. x.
46. Ibid., p. xix.
47. Gillispie, Genesis and Geology, p. 44. He goes on to say Miller was the only one who 'patiently put aside the conventional and arid argument from design as irrelevant ... The issue to him was ... "a belief in the immortality and responsibility of man, and



in the scheme of salvation by a Mediator and Redeemer...'

(p. 176). As we have seen, these were actually virtually the same as Sedgwick's concerns.

48. Ibid., p. 167-68.
49. The full Sedgwick quotation is given on pages 241-242 and 182 of this thesis. Gillispie does not indicate the context and omits several sentences including the key paranthesis.
50. Gillispie (ref. 47), p. 165.
51. Ibid., p. 150.
52. Hooykaas, The Principle of Uniformity, p. 221n, etc.
53. The irony of this is that the same group of modern scholars who see Sedgwick as associated with a liberal Anglican 'Cambridge Network' influenced by Coleridge, seem to see Sedgwick's major objection to the Vestiges as being transmutation as such. Yet Coleridge's dynamic version of science (for which see in particular Levere, Poetry Realised in Nature; Muirhead, Coleridge as Philosopher, p. 130f.) was favourable to the idea of transmutation.
54. Here Brooke gives a note to Gillispie.
55. Brooke, in Images of the Earth (Ed. Jordanova and Porter), p. 50.
56. Thackray and Morrell, Gentlemen of Science, p. 245.
57. E.g. Vestiges, p. 158: 'When all is seen to be the result of law, the idea of an Almighty Author becomes irresistible...'
58. Even aside from Continental scientists, it appeared in the works of Erasmus Darwin (see also Clark & Hughes, 2, p. 411).
59. At this time even the Bible Society could not find an acceptable common format for an interdenominational prayer meeting.
60. R. Young, 'Natural Theology, Victorian Periodicals and the

Fragmentation of a Common Context', in Darwin to Einstein...

(Eds. C. Chant and J. Fauvel).

61. Brougham, Discourse, sec. III V.1; and note 4.

62. Sedgwick (ref. 23), p. 13.

63. Originally 24th March 1860, repeated with corrections 7th April.

The article is reprinted by D. Hull in Darwin and His Critics.

Hull's comments on it, however, are a mystery. He says: 'In

Sedgwick's view, Darwin's theory was damnable on two counts:

It was radically new and it openly conflicted with Scripture.'

(p. 168). Sedgwick did not believe it radically new, and nowhere stated that it conflicted with Scripture. His objection to its 'materialism' was not a question of Scripture, but of his philosophical dualistic understanding of the nature of man.

64. Clark & Hughes, 2, p. 357-58.

65. In this connection one may question the view of T. Cosslet (in Science and Religion in the Nineteenth Century, p. 2) that the controversy was between 'a science pursued in the interests of natural theology, that relates its findings to moral and religious values, and a new, professional "value free" science.' The argument that Darwin's views were value-free is implausible (the quotations she gives from Darwin are as metaphysical as those of Miller), and her emphasis both on professionalisation and on natural theology is questionable. I would, however, accept at a deeper level the suggestion by C.A. Russell (Science and Social Change 1700-1900, p. 258) that issues of 'cultural leadership' were involved, as science (in some senses, and in spite of piety of some scientists) replaced religion as a dominant mode of thinking.

66. Clark & Hughes, 2, p. 359.

67. Ibid., p. 360.

## (6) CHURCH PARTIES' ATTITUDES TO SCIENCE

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### 6.1 Attitudes to Science and the BAAS

#### 6.1.1 Recent Theory on Natural Theology in Early BAAS

The 1830's were very interesting and exciting years for anyone interested in the relationship between religion and science. It was really a heyday for natural religion. The Bridgewater Treatises were coming out, including Chalmers' and Whewell's (both 1833), and Buckland's (1836). Brougham published his Discourse (1835), and Sedgwick his Discourse on the Studies of the University (1833). Some of the land's acknowledged intellectual leaders were, in other words, arguing strongly for natural theology.

The second feature which makes the period interesting is the emerging self-consciousness of science as an activity. The British Association for the Advancement of Science (or BAAS) was founded in 1831, bringing an aspect of the travelling circus to science which had never been matched in the more sober discussions of (say) the Geological Society of London (founded 1807).

In geology there was a certain feeling that the science had 'come of age'. Accredited practising geologists formed an indeniable group, with accepted paradigms. Though controversy, both theoretical and on specific issues, continued, any 'outsider' would find ranks closed against him. Wernerism was a spent force, and the old style catastrophism in the style of Buckland and Cuvier was dying (Sedgwick renounced it in 1831, Buckland in 1836). The Principles of Lyell, whilst few geologists might accept his hyper-uniformitarianism, made geology readable to the general public, and heralded a new phase in the debates on the issues of Genesis and geology.

Now it seems to be assumed or argued by some recent writers that a growing self perception of scientists (as represented by the BAAS) as a group engaged in a godly but autonomous activity, was accompanied by a feeling of being under threat from religious quarters. Such writers may explain the emphasis within the BAAS on natural theology as an attempt to counter this perceived threat. Thus Thackray and Morrell write:

'Another important function of natural theology was that it could be used to defend the Association against religious critics. All Christian and clerical opposition could safely be ascribed to 'bigotry', as long as the BAAS had its own forms through which loudly to extol the Deity. Natural theology thus blunted criticism while serving to rebut the allied charge that science led inexorably to impiety and atheism.'<sup>1</sup>

The implication seems to be that these early nineteenth century scientists felt under threat; they had to watch themselves very carefully and make as many pious pronouncements as possible to 'draw the fire' of critics.

The continuing emphasis on natural theology within early BAAS has, then, been suggested to have two functions, as we have seen above and in the last section:

- (i) Blunting possible clerical criticisms
- (ii) Acting as a 'mediating influence' between different denominations in BAAS.

The present section of the thesis aims to examine the plausibility of these suggestions, with particular reference to Sedgwick as one of the leading early BAAS figures.

To answer the first suggestion we shall have to ask a number

of questions about each theological group's attitudes:

- (a) What was their attitude to science itself ?
- (b) What was their attitude to specific aspects of science, in particular to the theories of geologists concerning origins ?  
Was there any call to limit or ban geological researches or to 'unfrock' geologists who were ministers ?
- (c) What was the attitude of the group to the BAAS as distinct from science as such during this period? On what was any opposition based?
- (d) In what sense might a natural theology (or more properly a physico-theology) blunt or deflect the group's criticism either of BAAS, geologists, or science?

The question of the supposed 'mediating influence' will be dealt with separately, in chapter 6.4 below.

#### 6.1.2 Alternative Attitudes to Science, Geology and the BAAS

The rest of this chapter aims to set out some possible alternatives of view on these three topics, which different church parties might have taken.

##### (a) Attitudes to Science Itself

Science could have been regarded as a danger to state or religion which ought to be discouraged. At the other extreme it could have been seen as a laudable study of God's works in creation, telling more of his wonders. At neither extreme it might be seen as an activity harmless in itself, but a possible distraction from the real business of religious life. Needless to say, opinion might in reality vary along a scale on which these three are simply points.

##### (b) Attitudes to Geology

Here there are a number of alternative views. Yule has given

a good breakdown of the possibilities, thus:<sup>2</sup>

1. Deny the truth of Genesis and get on with geology:
  - 1.1 Positively deny Genesis (few authors)
  - 1.2 Practically ignore Genesis (many secular geologists)
2. Deny the truth of geology and be left with Scripture (Cole)
3. Deny that there is any connection between Genesis and Geology:
  - 3.1: absolutely - 3.11 and see Genesis as myth (Geddes & Powell)
  - 3.12 and retain both ('schizoid reaction')
  - 3.2 but allow that there is a general analogy between both  
(Winning, Miller, in general vogue after 1840)
4. Fit geology to Genesis 'Scriptural Geologists'
5. Fit Genesis to geology: 'Geological Hermeneuts'
  - 5.1 adopting F-thesis (after Faber) (i.e. the 'age-day' thesis)
  - 5.2 adopting I-thesis (after Chalmers) (i.e. the 'gap' thesis)

In general this schema (copied exactly but with one or two explanations added from Yule's own text) is useful. One might wish, however, to question some details, e.g. the positioning of Miller who was surely saying something more than mere 'general analogy'. It is also rather puzzling as to who (in England at any rate) the 'many secular geologists' were in this period. Most of them seem to have been clerics or at least expressed some interest in the problem (e.g. Lyell).

At this point a few brief comments may be relevant. First, the schemes of the Scriptural Geologists varied, but most included a universal flood.<sup>3</sup> Usually they did not attack science as such, but rather said that the geologists were wrongly applying it. More subtly, many accepted the geological 'facts' (i.e. the strata) but

denied the 'interpretation' (i.e. the time periods). Often they would deny that they were fitting geology to Genesis, claiming that even independently geology would give rise to their scheme. Yule is not, therefore, giving a description based on what they thought they were doing, but on what we judge them to have been doing. On the other hand 'fitting Genesis to geology' was broadly what Chalmers and Faber in section 5 thought they were doing. The age-day theory is traced by Millhauser back to Buffon, but it had been made available to our period in G.S.Faber's Genius and Object in 1823. The theory that geological strata were to be fitted into a 'gap' somewhere in Genesis 1-2 came from Chalmers: The Evidence and Authority of the Christian Revelation (1817). The 'gap' theory was more popular than the age-day theory, being supported by Buckland and Pusey from 1836, Sedgwick, Phillips and later Faber himself (though Miller supported the age-day). Both theories (and there were various versions of each) implied either a localised flood or one which was purely miraculous and left no traces.

### (c) Attitudes to the BAAS <sup>5</sup>

It seems clear that someone could approve of science but dislike the BAAS, depending on his perception of its intended function. In particular I suggest three basic possible attitudes to it:

- (i) Love it all, seeing the razzmatazz of the BAAS as an essential part of popularising science (e.g. Brewster).
- (ii) Like the idea of a scientific association, but regret the circus element of the BAAS (e.g. the Times ?).
- (iii) Dislike the razzmatazz, but, more significantly, question the need for any such meeting of scientists, and see dangers to religion if such a meeting be inter-denominational (e.g. Bowden and Newman).

### 6.1 Notes

1. Thackray and Morrell, Gentlemen of Science, p. 229.
2. J.D.Yule, The Impact of British Religious Thought in the Second Quarter of the Nineteenth Century, Table 9.1 (p. 324).
3. In this thesis I use the phrase 'Scriptural Geologist' after the manner of Yule (ref. 2) and M. Millhauser (e.g. 'The Scriptural Geologists, An Episode in the History of Opinion', Osirus, 11, 65-68). A recent book by T. Cosslet (Science and Religion in the Nineteenth Century) classes Sedgwick, Buckland, Chalmers and Miller unequivocally as 'Scriptural Geologists' (e.g. p. 4). Not only is this likely to be confusing in view of the previous usage of the term, but it is not in any case very satisfactory. Would we today (for example) call someone like Professor C.A. Berry a 'Scriptural Geneticist' because he seeks to reconcile his work on genetics with a belief in the Bible? Surely this would be very misleading; but it is no less misleading to call men like Sedgwick, Buckland and Miller 'Scriptural Geologists', and so I shall not do so but will keep to the previous use of that phrase. As for Chalmers, he surely would not have claimed to be any kind of geologist, nor to be making any contribution to geology.
4. M. Millhauser, Just Before Darwin, p. 42.
5. References to the views of those named in brackets will be made in the course of the rest of section (6).



6.2.1 Recent Scholarship and the Issues

Brooke, in an article concerned with mid nineteenth century natural theology, made the following comment:

'In works of considerable sophistication the evangelicals are still dismissed en bloc as bibliolaters who viewed the sciences with suspicion.'<sup>1</sup>

Brooke was concerned with works making explicit generalisations about Evangelicals. Other works, however, may be less explicit and yet leave an impression that Evangelicals had little interest in or commitment to science, other than in inventing schemes of Scriptural Geology. This point may be illustrated from Gentlemen of Science by Thackray and Morrell. Their section headed 'Science and Religion' is prefaced by a quotation from Simeon 'the leader of the Evangelical Revival in the Church of England'.<sup>2</sup> The quotation is 'Investigate the works of creation; it cannot do any harm. But beware of feeding upon science, lest your souls be starved.' Thackray and Morrell give no further explanation of the views of Simeon or his Evangelical Associates on science, and pass on to portray the early BAAS (in the 1830's) as 'dominated by liberal Anglicans'. They state:

'Those Christian groups professing a firm commitment either to the literal words of Scripture or to the magisterial tradition of the Church were notable both for their absence from the British Association and for their growing attacks on it. In contrast, the liberal Anglicans found strong allies in those sections of the church least given to doctrinal formulas. The Quakers and the Unitarians each provided one among the twenty-three Gentlemen of Science.'<sup>3</sup>

The first sentence of this quotation makes deliberate reference to

'Christian groups', and the alternative commitments mentioned are presumably intended to identify such 'groups' or 'parties'. In such a context we must surely be intended to take a commitment to 'the literal words of Scripture' to refer to Evangelicals, and one to 'the magisterial tradition of the Church' to the High Church ? Evangelicals, therefore, as a Christian group, are notable for their growing attacks on the BAAS. There is no indication from Thackray and Morrell that a powerful section of moderate Evangelical opinion, in England as well as in Scotland, was highly critical of such attacks.

Thackray and Morrell continue their account in similar style. When BAAS spokesmen have made their case for 'science as a Godly, yet autonomous, intellectual pursuit', then: 'The first reaction came from an evangelical Scriptural Geologist, Frederick Nolan.'<sup>4</sup> A few pages later Thackray and Morrell put it more generally: 'Evangelical Christians, given to a literal interpretation of Genesis, led the assault [on the authority of the BAAS]'.<sup>5</sup> On the next page, after describing Buckland's efforts to get Sedgwick to reply to Nolan, they state: 'In practice, vindication of liberal Anglican views and of the BAAS came from Powell and Daubeny...'<sup>6</sup> This sounds as though the views of Powell and Daubeny epitomised those of 'liberal Anglicans' in general, including Sedgwick. After describing the attacks made by Powell and Daubeny on Nolan, Thackray and Morrell write:

'However, the evangelical Christian Observer pointed to the absurdity of arguing that divine revelation is to be credited in its moral but not in its physical statements, and accused Daubeny of doubting the inspiration of the Old Testament. The Reverend William Conybeare, who had presented a report on geology to the 1832 Meeting, sprang to Daubeny's defence with

an epitome of the liberal Anglican position: 'the Bible is exclusively the history of the dealings of God towards men.'<sup>7</sup>

A very clear impression is created here of the Christian Observer, the mouthpiece for the most important section of Anglican Evangelicals in the period, making an attack on the views of the liberal Anglicans of the BAAS, and meeting a counter attack from Conybeare. We shall consider later in the chapter how true a picture this is of the incident concerned. Thackray and Morrell, however, continue their chapter with the apparent implicit assumption that opinions on the issues divided very much along (church) party lines - with liberal Anglican opinions set against Evangelical ones. Thus, when Powell's response to the Scriptural Geologists was to suggest that 'Genesis could not be regarded as a historic narrative', Thackray and Morrell see no basic difference between his position and that of other liberal Anglican divines (presumably including Sedgwick). Rather, they treat the matter merely as a question of 'tactics' and greater daring on Powell's part.<sup>8</sup> Thomas Chalmers is the only identified Evangelical whom Thackray and Morrell present as having a positive attitude to science, and even his presence is somehow presented as a confirmation of 'the close alliance between the British Association and the liberal natural theologians.'<sup>9</sup>

In summary, therefore, the impression left by Thackray and Morrell is surely this: Evangelicals as a group, and including the Christian Observer as the mouthpiece of moderate Evangelical Anglicans, took the side of the Scriptural Geologists and were hostile to the BAAS; on the other hand the leaders of the BAAS were united in their liberal Anglican theology and differed only as to 'tactics'.

The present chapter of this thesis will present some of the

evidence from which it may be judged whether this is an impression which truly portrays the situation in the early nineteenth century.

### 6.2.2 Evangelicals' Attitudes to Science

In chapter 2.1 we considered the origins of the Evangelical revival in the eighteenth century, with Wesley and Whitfield among the key figures. A recent work by C.A. Russell has illustrated how positive were the attitudes of these Evangelical pioneers towards natural philosophy.<sup>10</sup> But what of Evangelicals at Cambridge, and in particular in Sedgwick's formative years there? Sedgwick arrived in 1804, and for the first two decades of the nineteenth century the three most influential Evangelicals in the university were probably Isaac Milner, Simeon and Farish. These will therefore now be considered.

Isaac Milner was Senior Wrangler in 1774, and became an Evangelical on a continental tour with Wilberforce in 1785. In 1781 he became the president of Queens, which he ruled despotically and turned into a recognised Evangelical centre. He also became Jacksonian Professor of Natural Experimental Philosophy 1783-92. Gunning described his lectures:

'He did not treat the subjects under discussion very profoundly, but he contrived to amuse us, and we generally returned laughing heartily at something... His experiments in Optics were very little more than exhibitions of the Magic Lanthorn on a gigantic scale... I cannot say that I benefitted much by my attendance at these lectures. I was subsequently nominated by my college to attend his Chemical Lectures (he being deputy for Dr Pennington). These, I understood from persons much better qualified than myself to judge of his merits, were very excellent...'<sup>11</sup>

Though Milner was a brilliant man, he made no profound contribution to research. It is, however, significant, that in days when many Professors failed to lecture at all, one of the two leading Evangelicals at Cambridge (the other in this period being Simeon) was committed to the teaching of science in the university. Milner continued to exert an influence (though he resigned from his Professorship) until his death in 1820.

We must remember, again, that in the late eighteenth century Evangelicals at Cambridge were very few. Yet, when Milner resigned in 1792, one of the two candidates for the Professorship was the Evangelical Farish. On that occasion he lost, but he then became Professor of Chemistry from 1794 to 1813, and later held the Jacksonian Professorship as well. He held lectures to exhibit 'the application of Chemistry to the Arts and Manufactures of Great Britain' from at least 1802, and Sedgwick (then age 29 but not yet a Professor himself) attended these lectures in Spring 1815.<sup>12</sup> During this period, however, Farish was also the close friend and confidant of Charles Simeon. In 1809, for example, Simeon was attacked in a pamphlet by the Christian Advocate Dr Pearson. Carus, in Memoirs of the Life of the Rev Charles Simeon, says:

'Mr Simeon prepared a reply. But previous to its publication he submitted it to the judgement of his faithful and clear sighted friend, the Rev. Wm. Farish... then Professor of Chemistry...'<sup>13</sup>

After receiving Farish's advice, Simeon apparently amended his reply. Farish, again, was one of the key figures in establishing the Bible Society in Cambridge in 1812.<sup>14</sup> All this shows Farish to have been one of the inner circle near to Simeon.

Farish continued to lecture, and his 'highly curious and useful course' is mentioned by the British Critic in its 1831 defence of English science in the 'declinist' controversy.<sup>15</sup> In 1833 he attended the BAAS, and gave an address:

'Professor Farish explained to the meeting the advantages which he conceived would be derived from applying the power of steam to carriages on undulating roads in preference to level rail-ways.'<sup>16</sup>

We have seen, then, that two of Simeon's closest and most important Evangelical associates at Cambridge were, for some time, Professors who lectured on scientific subjects. This must colour our understanding of his own attitudes to science, especially since his explicit pronouncements on it were few, and often come to us second hand. We also need to bear in mind that Simeon himself was a single-minded person who was devoted to his special calling in Cambridge, and yet he is aware that his own station may have obligations in it which do not apply to those placed by God in other situations.<sup>17</sup> In a letter in 1822 Simeon states that he considers Chalmers as 'raised up by God for a great and peculiar work'.<sup>18</sup> In the same letter, Simeon confesses: 'Religious people are apt to overlook secular matters, instead of giving them a due measure of attention.' He adds, however, 'My province is just to attend to the little things that are before me.'

Comments of Simeon specifically on science are rare. He admired Newton (though presumably knew nothing of Newton's theology), and in a letter of 12th June 1822 expresses satisfaction that he had been able to furnish Whewell with an original portrait of Newton to hang in the college.<sup>19</sup> In 1833 Simeon (aged 73) attended some of the BAAS meetings in Cambridge. He commented casually in a letter shortly afterwards:.

'We have had a large assembly of philosophers here, both British and Foreign, and their exhibitions have gone off well. But one atom of heavenly science is in my estimation preferable to all that they brought forth. And so St Paul thought, when he counted all but dross and dung for the excellency of the knowledge of God.'<sup>20</sup>

At first sight this might seem a puzzling comment for a man whose close friend and confidant for over twenty years held a Chair in the sciences and had actively participated in the aforementioned 'exhibitions'. But two things need to be noted. Firstly, the quoted words of St Paul come from a passage where the latter is arguing that natural advantages, though good in themselves, are worse than useless if they are seen as an alternative to faith in Christ.<sup>21</sup> In another place, as Simeon would well have known, Paul answers the question: 'What advantage then has the Jew' by saying 'Much in every way...'<sup>22</sup> Simeon is not denying the usefulness of science, but setting his priorities. The second point is that Simeon may be intending a pun. The word 'science' means 'knowledge', so Simeon is saying that 'heavenly knowledge' is preferable to all that brought forth. But 'knowledge of God' as Paul meant it referred, of course, to far more than intellectual knowledge about God: it included experience.<sup>23</sup> Simeon certainly is not saying that he would rather see a British Association for the Advancement of Theology instead.

What is interesting, also, is that his first biographer and fellow Evangelical William Carus, writing in the later 1830's and as Simeon's successor at Cambridge, felt it best to soften the apparent severity of the above quoted remark by a footnote as follows:

'Mr. Simeon, however, was by no means indifferent to the advancement of science. and had great pleasure in attending these meetings of the British Association. In his occasional seasons of leisure he had peculiar pleasure in reading works of a philosophic character; and when recovering from a severe illness, a year after this, the volume which he enjoyed most, next after the Holy Scriptures was the Bridgewater Treatise by Dr Whewell, on Astronomy and General Physics; which was read over to him more than once, as he was able at intervals to hear it.'

Carus, who was in fact also a friend of Sedgwick, shows by this that he himself was certainly favourable both to science and to natural theology; this in itself is important. But it is difficult to believe that Carus, writing at a time when many of Simeon's circle were still living, would have made such a comment without at least some foundation of truth.

Simeon also commented specifically upon geology:

'Geologists take too much upon them. They have too few facts to reason from, and the subject is as yet too little known for them to make such confident assertions as they do. They have examined only the surface of the earth, and only parts of that surface... We are fond of any favourite system, and good men have got hold of geology and ridicule any attempt to reason them out of it. But mere plausibility must not carry all before it... Investigate the works of creation; it cannot do any harm. But beware of feeding upon science, lest your souls be starved. Faber's idea of each day of creation being a thousand years seems little better than nonsense. Some would explain the opening of



Genesis by saying that God first created the substance of the earth; and that while it was 'void', or in a state of confusion (which it is asserted the word means), darkness brooded over it, and then God created the light. This interpretation may be true, though if it be the Christian gains nothing by it. But if it may be asserted that the contrary of this view is not necessarily true, we take from the sceptic one of his weapons; and therefore the idea, if correct, is valuable.<sup>24</sup>

This passage, based on much later recollections, seems to be making reference to the age-day and the gap theories. Simeon does not seem to reject the gap theory (if this is his meaning) as unscriptural. But he was not, personally, greatly interested.<sup>25</sup> His follower and successor, Carus, made it clear in his 1889 article on Sedgwick that he for one was unprepared to reject the assurances of his friend Sedgwick that geology was compatible with Christian faith.<sup>26</sup>

It might, perhaps, be conjectured that Simeon, in spite of his friends' commitments to science, might by his negative attitude discourage his disciples from pursuit of science. The plausibility of this conjecture is, however, called in question by the case of Charles Cornelius Gorham. Gorham became an Evangelical whilst at Queens, and was third wrangler in 1809. His Evangelical commitment was strong even at this stage<sup>27</sup>, and Gorham, who was evidently very much prepared to stand up for his beliefs, later championed the Evangelical cause in his famous controversy in 1847-8 with the High Churchman Phillpotts. Yet, in 1818, he stood against Sedgwick for the post of Woodwardian Professor of Geology - with the promise that, like his friend Farish, he would

give a course of lectures if elected.<sup>28</sup> He also claimed: 'I feel a conviction that few people in the University have followed up the Science more sedulously than I have...', and had formed a valuable collection of plants in a Swiss tour in 1810-1811.<sup>29</sup> The case of Gorham shows that a young man of considerable talent, who associated himself with the Evangelicals in Cambridge, could find it quite natural both to pursue science and to seek a scientific Chair. Evangelicals Farish and Pryme both voted for Gorham, as did the Professor of Mineralogy, E.D. Clarke, at whom we shall presently look.<sup>30</sup> All this makes it clear that the Simeon circle regarded commitment to scientific study as a worthy one.

A final person who requires consideration is E.D. Clarke. In 1807 he began a course in Mineralogy, the first of its kind but well attended, and he was appointed to a Chair of Mineralogy from 1808-1822.<sup>31</sup> He spoke extempore and enthusiastically, and Sedgwick attended his course, later testifying: 'E.D. Clarke was a benefactor, and how. He gave a start - he kept us awake etc...'<sup>32</sup> It is difficult to say exactly what religious position Clarke had. He did not align himself with Evangelicalism in as clearcut a way as Milner, Farish and Gorham. What is interesting, however, is that in 1812 he risked criticism and possible loss of preferment by firmly supporting, with Farish, the establishment of a Bible Society at Cambridge.<sup>33</sup> The Bible Society was mainly supported in those days by Evangelicals, and in this respect at least Clarke was willing to identify himself with them.

Let us remember that at this time Evangelicals in Cambridge were not numerous. On the other hand, neither were those who actively practised or taught natural science. Thus Cannon writes:

'Isaac Milner as Jacksonian Professor of Natural Experimental Philosophy did much, from 1783 to 1792, to demonstrate electrical, chemical, and mechanical devices and experiments; and the tradition was continued by William Farish from 1795. Farish lost the election for the Jacksonian chair and had to be contented with the chemistry one; but as the Jacksonian winner, F.J.H. Wollaston, wanted to lecture on chemistry, Farish lectured on machines and mechanism. "It seems fair," says the historian of Cambridge engineering, "to claim this as the first real course in mechanical engineering in any British university.

Another of these individual enthusiasts was Edward D. Clarke...<sup>34</sup>

Cannon fails to note that of the four he mentions, two were leading Evangelicals, and a third had sympathies with the largely Evangelical Bible Society. Those of Cannon's viewpoint have tended to emphasize the (largely speculative) interest of Coleridge in science; they tend to pass over without comment the level of Evangelical involvement with science which (considering the numbers of declared Evangelicals) is quite astonishing. They also tend to ignore the fact that Sedgwick's own formative contacts with empirical science were at the hands of Professors with Evangelical sympathies, and his rival for his own post was also an Evangelical.

One last point concerns the Cambridge Philosophical Society, started in 1819. The early leaders in this were Sedgwick, Henslow, Farish and Clarke.<sup>35</sup> Of these, again, we know the Evangelical

connections of Farish and Clarke; this thesis has argued those for Sedgwick, and Henslow never seems to have been associated with any kind of 'liberalism' in theology.<sup>36</sup>

With Oxford University we are less immediately concerned, but some comment may be useful as we build up a general picture of Evangelical attitudes to science. In this period there was no 'Simeon' at Oxford, and both Evangelicalism and science were weaker than at Cambridge. J.S. Reynolds counts a total of only 37 Evangelicals admitted to fellowships during 1807-1845 and comments: 'The majority of fellows of colleges at this time were high churchmen of the traditional kind.'<sup>37</sup> Science, also, had never been as strong as at Cambridge, and in the 1830's was in decline. F. Sherwood Taylor says: 'Thus in the later 'forties the scientific instruction of the (Oxford) University was given by four men, Daubeny, Buckland, Walker and Acland.'<sup>38</sup> What is interesting is that in a time when Evangelicals were few in number, one of these four was an Evangelical, Robert Walker.

Born in 1802, Walker took a first in 1822, M.A. in 1825 and from 1826-31 was a chaplain in the Evangelical stronghold of Wadham college. He was also a tutor and from 1828-48 a frequent examiner in mathematics in this period. He was Professor of Experimental Philosophy 1839-65.

Interesting parallels might be drawn between Walker and the earlier Milner, Farish and Clarke at Cambridge. Like them, he made no major contribution to research, and his value (again like theirs) was in the introduction of science teaching into university life. Thus William Tuckwell says of him:

'(he) constructed and exploded gases, laid bare the viscera of pumps and steam engines, forced mercury through wood blocks

in a vacuum, manipulated galvanic batteries, magic-lanterns, air guns... Walker was a man of great ability, the first, I believe, to introduce into Oxford the analytical as distinct from the geometrical, treatment of higher mathematics. He was also a notable preacher of the Evangelical school; his sermons pure in style, and reflecting strong personal piety.<sup>39</sup>

He also addressed a pamphlet in 1848 to the Vice Chancellor on the need for all students to attend a compulsory science course. This shows the depth of his commitment to science.

Walker played no great part in the BAAS, though this could partly be explained by his contribution being in teaching rather than in research. When the BAAS met at Oxford in its second meeting (1833) its Report contains in its Committee of Mathematics and General Physics the name of the Rev. R. Walker FRS, so he clearly had some sympathy with its aims. Canon, again, mentions Walker 'By the 1840's the new methods were even slipping into Oxford, through the teaching of the notable evangelical preacher, and reader in experimental philosophy, the Reverend Robert Walker.'<sup>40</sup>

Again, then, though Evangelicals were few in number we find one of them pioneering in the teaching of science in universities.

Of the others of the 'four men of science' mentioned above, Buckland's connections are also of interest. His friendship with the High Church and Tractarian leader Pusey is well known, and Pusey's support for his geological views was given in a lengthy footnote in his Bridgewater Treatise.<sup>41</sup> Buckland also refers to some 'excellent articles' on the issue in the Evangelical Christian Observer.<sup>42</sup> Indeed, his family connections seem to have included

both Pusey and Evangelicalism. His wife received 'much comfort' from Pusey's spiritual ministrations on the death of her nine year old son in 1847.<sup>43</sup> The church the Buckland family attended, however, was that of an 'excellent evangelical preacher' Rev. F. Waldegrave and Mrs. Buckland was 'an assiduous worker in Mr. Waldegrave's poor parish'.<sup>44</sup>

In the period, then, up to the 1830's and into the 1840's there is evidence that, far from any suspicion or animosity, known Evangelicals were in the forefront of bringing science teaching to universities. The virtual leaders of Evangelicalism in Scottish and English academic circles (Chalmers and Simeon) both attended the 1833 BAAS meetings, and Simeon's close friend Farish participated. Would Sedgwick (who was a friend of Simeon's disciple and later successor Carus, and associated with Farish as a co-founder of the Cambridge Philosophical Society) really feel that either the BAAS or science had anything to fear from such moderate Evangelicals ?

### 6.2.3 Genesis and Geology

In the quotation made from Thackray and Morrell at the beginning of the present chapter, mention was made of the 'Scriptural Geologists', said by Thackray and Morrell to have 'enjoyed a vigorous phase in the 1830's.' Thackray and Morrell (as a number of others) associated Scriptural Geology with the Evangelicals and seemed to imply that it was a fairly general Evangelical viewpoint. For this reason the issues of Scriptural Geology have been placed in this chapter on the Evangelicals, though (as we shall see) any association of Scriptural Geology with Evangelical orthodoxy would in fact be misguided.

Yule's scheme defined 'Scriptural Geologists' as those who fitted geology to Scripture. Some of them may, in fact, have accepted that they were doing this; others, however, might have denied it and claimed rather that, independently of Scripture, the observed facts of geology would lead naturally to their system. They were, however, united in a rejection of 'orthodox' geology, and the espousal of some 'new' radically different schema. Most of them involved the agency of a universal flood or floods to lay down large thicknesses of strata - a theory abandoned by 'orthodox' geology many years earlier.

The following are some of the most important works of Scriptural geology:

Andrew Ure: A New System of Geology (1829)

Sharon Turner: The Sacred History of the World (1832)

W.M. Higgins: The Mosaical and Mineral Geology Illustrated and Compared (1832)

Frederick Nolan: The Analogy of Revelation and Science (1833)

Henry Cole: Popular geology subversive of Divine Revelation (1834)

George Young: Scriptural Geology (1838)

Dean Cockburn: A Remonstrance to the Duke of Northumberland (1838)

A Letter to Professor Buckland (1838)

The Creation of the World (1840)  
(dedicated to the geological society)

The Bible Defended Against the British Association  
(1845)

A New System of Geology (1849)  
(Addressed to Sedgwick)

Of particular interest to us are those of Ure, Nolan, Cole and Cockburn, because they have direct or indirect links with Sedgwick; all these will be considered later. To begin with, however, let

us consider the general attitudes of the Evangelical world to the issues.<sup>45</sup> Yule has pointed out that the Calvinistic The Record, tended during the period to support Scriptural Geologists, whilst the moderate Christian Observer did not.<sup>46</sup> But it will be useful to begin the present study by looking in more detail at the exact position of the Christian Observer, remembering its importance as a voice of moderate Evangelical Anglicanism. Then we may consider the actual theological and scientific positions of each of the Scriptural Geologists important to our theme, and how they related in particular to Sedgwick.

The Christian Observer first reviewed Chalmers' 'gap theory' as it appeared in his Evidences and Authority of Christian Revelation in April 1815<sup>47</sup> in the very earliest part of Wilks' editorship. The review was favourable, and the gap theory became one acceptable belief amongst moderate Evangelicals. Also influential in this period was an appendix to John Bird Sumner's Records of Creation (1814) on geological issues. Sumner carefully advanced the point that Moses did not intend to teach geology, and the version of the gap theory he suggests is consistent with Scripture, and is one which incorporates the then geologically accepted Cuvierian diluvialism. During the 1820's the Christian Observer maintained its balanced and non-dogmatic approach - even hesitating at whether Buckland claimed too much for his diluvial theory!

As we get to the 1830's, we find little about science in the 1831 numbers. In October 1832 it laments the death of Cuvier:

'he refuted certain naturalists, who, to overturn the doctrine of final causes teach that there was originally but one kind of animal, a mere chance in the infinite mutations of matter



in eternal ages, and that from this have arisen all others.... Man is thus associated with a mere worm, or polypus, which happens by chance to have been more developed than his kindred worms, or polypi, and owing all his superiority to a happy accident. Cuvier's demonstrations overturned this absurd theory, and formed an excellent cause of natural theology, the more valuable as being the result of independent science.'

This passage shows no disposition to bind science to Scripture, but rather a rejoicing that science independently has cut the physical grounds away from a wrong metaphysical system.

In its November 1832 issue, the Christian Observer reviews W.M. Higgins: The Mosaical and Mineral Geology Illustrated and Compared. In the course of the review the editorial recounts the two main theories (i.e. the age-day and the gap theories) referring back to previous reviews of Faber's arguments. The editorial makes no recommendation between them, but notes that in either case geology does confirm Genesis in making an assumption that man is recent. It goes on:

'These important corroborations being furnished by the science of geology, we are not to be alarmed because the same science also intimates certain difficulties, which seem to require some correction of the popular arguments on the first chapter of Genesis - not any alteration in the text but a variation in the interpretations... The narrative (of Genesis) is much too succinct to ground on it all the physical details of the creation.'<sup>49</sup>

The Christian Observer was still prepared to print letters from readers of other persuasions (e.g. in 1833 it printed a letter from a Hutchinsonian<sup>50</sup>) but it had made its sympathies clear. In 1833 it

contained little which related to science.

In 1834 there arose the controversy which was referred to by Thackray and Morrell in the section quoted from them at the beginning of this present chapter. In April the Christian Observer carried a letter from 'Layman' 'On the Infidel Tendency of Certain Scientific Speculations.'<sup>52</sup> This defended Nolan and attacked Daubeny's article in the Literary Gazette; it also made an attack specifically on Sedgwick, quoting his 1830 Presidential Address to the Geological Society of London.

'Layman' regards Daubeny as a 'sort of paraphrase' of Sedgwick.

'Layman' carried no more authority than any other correspondent; but was it (as Thackray and Morrell) seem to imply, the editorial line as well ?

The editorial line is actually very careful:

'A more daring and absurd proposition was never invented, than that a Divine revelation is to be credited in its moral but not in its physical statements; and we do not believe that any man who so asserts has the slightest faith in the Bible as a Divine revelation in either department. A large number of geologists, as well as of other scientific and unscientific men, are, we feel, infidels - or at least sceptics - either avowed or concealed: but a broad line of distinction ought to be made between such men and those who, agreeing with them in their physical facts, yet differ from them in their sceptical opinions; and who consider that the word of God may be, and ought to be, so construed as without violence to reconcile the scientific theory with the sacred text. Our correspondent's paper, if read without this fair distinction... would class the sceptical writers and writings to which he alludes, with

the writings of many undoubted Christians, including the geological lectures [sic] at both our Universities. We may doubt certain alleged physical facts, or certain theories supposed to result from them; but if another man, who believes both, thinks that the word of God, rightly interpreted, is perfectly reconcilable with them, he... cannot in justice be treated as an infidel.'

We may note a number of things here. Firstly, the Christian Observer does not see the issue either as the authority of the BAAS or as the truth (or otherwise) of the theories of geology. Its disagreement with Daubeny (who is not specifically named) is about theology, not about science, and it concerns whether or not physical statements of the Bible are trustworthy. The Christian Observer is very careful to emphasize that theological scepticism on this point may be present in both scientists and non-scientists, and the science is incidental. It further maintains that others (and it instances Sedgwick and Buckland) do accept that physical statements in the Bible, rightly interpreted, are trustworthy, and that these others may well accept the same geological facts as the sceptics. After the passage quoted it expresses fear that 'Layman's' failure to make the correct distinction 'might give pain to some of the most pious and excellent men of the age' to whom his criticisms do not apply. It also defends the age-day theory against Layman's criticisms.

In May the Christian Observer carried some comments on these issues by W.D. Conybeare.<sup>53</sup> Conybeare had noted Layman's 'marked reference' in introducing his topic to those 'who are even servants of the church of God.' He refers this to Buckland, Sedgwick and himself, and it is these particularly whom he wishes to defend.

His defence is basically to deny that any of them ever held such a position as Layman suggests. If (say) Buckland did not believe Scripture to be trustworthy in its physical statements, then he would have been inclined 'to disconnect his geological speculations from the records of Scripture.' Such an accusation, to Conybeare, is absurd. Layman, moreover, has 'completely misapprehended' Sedgwick's Presidential Address. Sedgwick meant only to assert what Layman himself believes, i.e. Scripture employs 'common language' and is not meant to teach us scientific fact.

Conybeare then refers to possible areas of conflict between geology and the Scriptural narrative. He says:

'...it is surely nowise inconsistent with the fullest reception of Revelation to maintain that it professedly confines itself to the exposition of the Great Creator as they concern his final intellectual creation: that, in a word, the Bible is exclusively the history of the dealings of God towards men.'<sup>54</sup>

In its context, the clause at the end of this passage suggests only the possibility of time dispensations before those of mankind, over which the Bible passes in silence: in essence the 'gap' theory. Conybeare then specifically goes on to defend both the gap and the age-day theory as 'methods of conciliation, either of which will obviously remove every appearance of discrepancy between the record of Genesis and our assumed geological periods.' We might, incidentally, note that had Conybeare really believed that the Bible was 'not to be credited in its physical statements' then such reconciliation would have been entirely unnecessary to him.

Finally Conybeare gives a somewhat half hearted defence of the anonymous writer in the Literary Gazette. Conybeare is 'convinced'

that the writer intended only to state the views he himself holds: 'though his expressions may possibly require to be more guarded. He is evidently no professional divine...'<sup>55</sup>

The Christian Observer again here makes editorial comment. It accepts most of Conybeare's comments, and refers back to its own distinction between Christian and sceptical geologists. It does, however, deny that the passages in the Literary Gazette can bear the interpretation Conybeare suggests, and it quotes two passages to illustrate this. It contrasts what is said about God withholding 'any extraordinary assistance from such portions as relate to phenomena in which man has no vital concern.' with a passage in Conybeare's own recent manual in which Inspiration implies 'not only are there "great truths" divinely revealed, but also "all the minutiae of things which they mention."<sup>56</sup> In short, Conybeare and Daubeny are poles apart.

The same issue of the Christian Observer carries a further attack by 'Layman', this time on Sedgwick. The editorial note, however, adds that 'Layman wholly misapprehends, in our idea, his meaning...'. It also publishes a longer defence of the geologists by another (anonymous) correspondent.<sup>57</sup>

At this point, then, we may briefly consider whether the passage quoted from Thackray and Morrell at the beginning of this chapter fairly represents the incident described above. We may note:

- (i) Conybeare's letter was criticising 'Layman', not the editorial line, and his own letter carried the same authority as Layman's.
- (ii) Conybeare's main object was to 'spring to the defence' not of Daubeny (who is defended rather half heartedly and with a patronising tone), but of the clerical geologists.
- (iii) The sentence Thackray and Morrell quote from Conybeare is

not by any means claiming that the Bible is accurate only in its moral aspects - it is speaking of time dispensations not recorded in Scripture.

- (iv) The latter position, if it really is 'the epitome of the liberal Anglican position', was actually championed by the Evangelicals Chalmers and Faber long before the BAAS was formed, and had received consistent support from the Christian Observer.
- (v) There was no 'liberal Anglican position' if this is supposed to include on the one hand Powell and Daubeny and on the other hand Sedgwick, Buckland and Conybeare. To one, reconciliation is pointless since the Bible is not to be credited anyway in its physical statements; to the other any difficulties of reconciliation are only temporary.

The following month, June 1834, the Christian Observer reviewed Henry Cole's Popular Geology subversive of Divine Revelation! A letter to the Rev. Adam Sedgwick. The review is unsympathetic:

'It is perfectly clear to us that Mr. Cole in his vituperation of Professor Sedgwick has not himself mastered the known facts of geology, or allowed his mind calmly to pursue them to their just and natural results, and then seriously considered in what relation they appear to the declarations of the Mosaic record as ordinarily interpreted.'<sup>59</sup>

Facts are always facts, however inconvenient, and the editor goes on with gentle irony:

'Let it be granted that the Geological Professors at both our Universities, with hundreds of pious clergymen and thousands of pious laymen who have studied geology, are infidels, either

directly or constructively, still this does not set aside those inferences from undeniable facts which many most excellent and sagacious Christians consider wholly irresistible.' Comparison is made to Newton's ideas and the earth's rotation, for similar adjustments to interpretation were needed there.

Powell's approach to the issues is criticised heavily. Quoting from Powell, the Christian Observer adds 'all this, we must say, borders upon the extreme proclivious verge of scepticism...'<sup>60</sup>

On Sedgwick, it is not afraid to make some criticisms of his theology - though these were based on a misunderstanding of some passages which Sedgwick himself was later anxious to clear up and over which he consulted the Evangelical Carus.<sup>61</sup> But on the question of geology and inspiration, the Christian Observer says:

'... it may be, that, chafed at the vexatious interference of men who are too ignorant or too prejudiced to weigh facts - contenting themselves merely with the easier task of denouncing their neighbours as infidels - he may have declared himself respecting the physical statements in the Holy Scriptures in a manner not to be commended... But we recollect no statement of his which goes any thing nigh the length of the passages above cited from the Geometry Professor of Oxford.'<sup>62</sup>

The Christian Observer would not actually commit itself to even a mild criticism of Sedgwick's views on these issues, and it regards it as a complete mistake to suppose that Sedgwick is to be placed in the same theological grouping as Powell. It then goes on to defend Sedgwick by showing that no less an Evangelical than Chalmers is 'even worse' than Sedgwick on the issues, for Chalmers accepts not only the gap theory but the possibility of pre-existent matter, thus 'out Sedgwicking Sedgwick'.

Cole was evidently furious with the Observer for defending Sedgwick, and his attacks brought the journal to defend itself in the July 1834 number.<sup>63</sup> To show its editorial fairness, however, in the August number it inserted a reply from Cole to Rev. H. Budd on a separate issue of the Church Service. The gentle irony with which Cole's letter is prefaced is all the more devastating for its gentleness:

'In inserting Mr. Cole's letter we must strongly express our regret at some of the epithets which he applies to members of the Church of England... If Mr Budd should think that the style of Mr Cole's paper is a little sharp towards himself, we would console him with the reflection that Mr. Cole has not denounced him, in pamphlets and in advertisements in the Times newspaper, as an upholder of infidelity and as guilty of other pravitities manifold, as he has done us, and wiser and better men, in the matter of Scriptural geology.'<sup>64</sup>

Wilks clearly had great respect for both the piety and the learning of Sedgwick, but had scant respect for either in Cole.

The Christian Observer continued to maintain its previous line, but we need pursue it no further. Sedgwick would have felt little need to fear anti-geological attitudes in the moderate Evangelical Anglicanism represented by the Christian Observer - indeed he would have recognised in it a position very similar to his own. Moreover, though the Record tended to support Scriptural Geology in this period, it did not represent the influence and respect of the group associated with the Christian Observer.<sup>65</sup> Scriptural Geologists in this period, though vociferous, were not a powerful group.<sup>66</sup>

Let us, nevertheless, look at a number of the Scriptural



Geologists, understand their positions, and why in particular Sedgwick reacted to various of them as he did.

We can, in fact, make a number of generalisations about them, or at least about the more 'respectable' amongst them:

- (a) they were not anti-science, and did not want science as such suppressed in any way.
- (b) they accepted the reality of the 'facts' discovered by geologists (often speaking respectfully of their work), but disputed the 'interpretation' (i.e. that the strata were laid down over long periods)
- (c) they set up their own 'interpretation' (which usually involved some or all the strata being laid down by a universal flood)
- (d) they claimed that this interpretation was the most scientifically plausible even apart from Scripture, and usually claimed only that the right interpretation might be consistent with (their interpretation of) Scripture - not that all the empirical facts could be obtained from Scripture.

Andrew Ure (the first among those named above) exemplifies all these points. He was certainly not 'anti-science', for he himself (in 1829) had held the Chair of Natural Philosophy in Anderson's Institution for 25 years, and had been actively involved both in teaching science and in scientific research, his special area being applied chemistry.<sup>67</sup> He was admired by E.D. Clarke, whom he had visited at Cambridge, though the general recognition of his talents was tempered both by his tendency to extreme polemics and his lack of care over assumptions.<sup>68</sup> In 1821 he published a Dictionary of Chemistry which went through a number of editions. In short, he was personally highly committed to science, and is described

by Farrar as 'probably the first person in Britain to earn his living' in the profession of a 'consulting chemist'. He was also committed to natural theology: 'I have always taken occasion to point out the beneficent design which the whole mechanism of nature displays...'<sup>69</sup> Ure was an F.R.S., and had been a member of the Geological Society for longer than Sedgwick.<sup>70</sup> Sedgwick's 1830 castigation did not prevent Ure from later joining the BAAS (in which Sedgwick was a leading light), nor from addressing one of its sections in 1833 on 'the Thermostat or Heat-Governor'.<sup>71</sup> Ure's theological and geological views did not prevent him writing respectfully to mainstream geologist Phillips in 1839 asking for advice on water hole boring.<sup>72</sup> Ure's commitment to science was unquestioned - though some might doubt his commitment to religion.<sup>73</sup>

Ure was not a potential supporter of any 'anti-science' faction. So why was Sedgwick so critical of him at such length in his Presidential Report to the Geological Society in 1830?

Sedgwick begins:

'.... during the past year there has been set forth, by one of our own body, "a New System of Geology, in which the great revolutions of the earth and of animated nature are reconciled at once to modern science and to sacred history;" and to this title I will venture to add, - in which the worst violations of philosophic rule, by the daring union of things incongruous, having been adopted by the author from others, and at the same time decorated by new fantasies of his own ...'<sup>74</sup>

The fact that an actual member of the society could adopt such unphilosophical methods really galled Sedgwick. The fact that in doing so he had the temerity to quote one of Sedgwick's favourite passages of the sacred Bacon made the crime the more heinous!<sup>75</sup>

Ure explained what were then called 'primitive rocks' in the style later adopted by Gosse (Omphalos 1857). God created them by divine fiat, looking as though they were old, or as Sedgwick says: 'in mere mockery of our senses'. Sedgwick cannot, of course, disprove this - philosophically Gossianism is unassailable - but is suitably sceptical.

When Ure comes on to secondary rocks, an area in which so much research had then recently been done,

'This part of the work appears not to contain one original fact or the result of one original investigation: and of this we do not complain. We have, however, a right to look to it for information which shall not repeat exploded errors; but shall make a near approach to the level of recent observations.'

Ure's work 'unquestionably' does not do this. Sedgwick catalogues its many errors, its data superseded as much as six years earlier, its inconsistencies with itself when different bits have been copied from different authors, etc:

'Errors like these are above every thing calculated to mislead men who are unpractised in geology ? ... in the conduct of this work, the author has shown neither the information nor the industry which might justify him in becoming an interpreter of the labours of others, or the framer of a system of his own. '76

It is simply not Ure's field, though men might be misled into taking his 'facts' as real.

Sedgwick asks:

'Are we then for ever to wander among the mere perplexities of details, and never to hope for any system which may combine them?'

Sedgwick thinks not. The principle of actualism ('Effects similar in

kind to those which are produced now, must in all former times have been produced by some corresponding power of nature') will lead to such a system.

Sedgwick at this time was in the throes of losing his diluvialism (renounced openly at the next Presidential Address.) But he recognises that the time for what he calls 'speculative geology' is coming, the building of a system using the data of empirical geology. When this is done it should be done not by just anyone, but by someone with a real grasp of the current facts of geology.

Sharon Turner is similarly respectful to the geologists. He accepts the three divisions then current: primitive, secondary and tertiary. It might just be noted, however, that at that time the old terms primary or primitive, secondary or transitional and tertiary (with their overtones of Wernerianism) were being transformed. In the early 1830's, as a result both of his conversion away from all forms of diluvialism and of his ongoing work in the Palaeozoics, Sedgwick was beginning to use the terms purely as a time scale and not referring to composition. During the early 1830's, therefore, the terms may mean different things to different people. Sharon Turner's view was that the primitive were laid down in six literal days, the secondary during the 1656 years between the Adamite creation and the flood, and the tertiary since the deluge.

Frederick Nolan's work was important because it had the status of Bampton Lectures. In fact, Nolan, a graduate of Oxford, was the first clergyman to be asked to give the Boyle Lectures (1814), and the Bampton Lectures (1833) and the Warburtonian Lectures (1833-36). The Dictionary of National Biography states that 'his views were evangelical'<sup>77</sup>, and is followed in this e.g. by Reynolds.<sup>78</sup>

Thackray and Morrell simply describe him as 'evangelical', giving the impression that he was typical of the class. But the 'orthodoxy'

of his Evangelical position is certainly questionable. Indeed, at a time when Simeon and his scientific friends Farish and Clarke were risking loss of preferment by founding a Bible Society in Cambridge in 1812, Nolan was writing a pamphlet against the establishment of such a society as detrimental to the Church of England<sup>79</sup> - a viewpoint repeated in his Bampton Lectures of 1833. His 1812 pamphlet argues that in the early church 'It was their custom to instruct the people in short formularies, containing necessary points of faith; but to make no further provision for the dispersion of the Scripture, than to provide for their being publicly read and explained in their churches ...'<sup>80</sup> Nolan objects that the Bible on its own will not enlighten anyone - for many have the Bible but remain in darkness. It needs to be explained, and so teaching should come before its distribution. Nolan attacks the King James version in the edition then current for having made alterations to accommodate the non-conformists. Most of all he objects to any kind of union between churchmen and Dissenters - let the Dissenters go off and expend their energies printing Bibles and converting the heathen and perhaps they will have less energy to disturb us at home! 'Better for us that they should thus occupy themselves and some thousands of their emissaries, who can never be so harmlessly employed at home.'<sup>81</sup> It was a mark of Evangelicals generally to support the British and Foreign Bible Society, and virtually all Evangelical leaders did so. Nolan's view is more like a High Church or a Coleridgean position than an Evangelical one. In contrast, we know that shortly after this Sedgwick was speaking publicly and eloquently at the Bible Society's Cambridge meetings.<sup>82</sup> Nolan also has an attitude generally to Dissenters which is unlike (say) Wilberforce, Simeon, Bickersteth, Shaftesbury or Sedgwick, and more like High Churchmen. He says of them: 'Great, therefore as their love is to

the Bible and all mankind; still greater is their affection for their errors [sic] and themselves: and yet greater than either their hatred to the Church and its power.' Nolan goes on to argue that a common Protestantism is no basis for anything. He defends Apostolic Succession: '... on the demise of the first Apostle, "one was ordained" directly to be his successor. Through the baptism administered by such persons, or those whom they dispute, we are grafted into Christ'<sup>23</sup>.

Nolan undoubtedly did have some 'Evangelical' views, as witnessed by his work The Evangelical Nature of Christianity (1838). He was also anti-Tractarian. Yet his Evangelicalism has a distinctively 'High Church' flavour. He stands less close to the views of Simeon, and the Clapham Sect, Wilks and the Christian Observer, than does (say) Sedgwick. In other words, Sedgwick was a more orthodox Evangelical than Nolan on a number of issues. This makes it difficult to see Nolan's attacks on the geologists as the attacks of 'Evangelicals'.

Nolan's book itself is a peculiar mixture of attitudes. Nolan is sometimes accused of outdated geology; this is not altogether fair. To be sure his main reference work was G.B.Greenough's A Critical Examination of the First Principles of Geology (1819). But he has also read or looked at Lyell's Principles of Geology (1830). Where he is really geologically deficient is in up-to-date general grasp of what is going on. The old Cuvier-Buckland type of diluvialism is dying away, and this is not the issue between the Lyellian uniformitarianism and the new style 'catastrophists' (though since so many modern writers have misunderstood this, Nolan may be in good company). Nolan still refers to 'Neptunists' and 'Vulcanists' (lecture 3). He thinks that diluvialism is still

geological orthodoxy. He is slightly muddled, but his geology is perhaps only about four years out of date.

More confusing is his approach to science and Scripture. He writes:

'When from some imperfection in our information and discernment, Religion and Philosophy are found to be opposed beyond the hope of reconciliation; I trust, that no debate as to which should give place can arise within these walls, in which I stand the unworthy advocate of Revelation.'<sup>84</sup>

This is eloquent, but it is nonsensical. If the apparent opposition were really due to imperfections in our information and discernment, then surely neither should give place? We should then surely (as Sedgwick said) simply wait for more perfect information?

This, really, illustrates the problem of the so called 'Scriptural Geologists'. It is scarcely possible to deny that the geologists are finding something in their researches. To suggest that, say, Satan put them there to mislead the faithful would have fallen on as deaf ears in the nineteenth century as ours today. So they were shut up to declaring that the geologists were interpreting their researches wrongly. Nolan does this also, though in a very scientifically naive way. In Lecture 3, he states that the successive cataclysms idea (which he takes for geological orthodoxy with the 'illustrious exception' of Greenough) cannot be conclusively disproved from Scripture, but he himself is unable to reconcile it. He refers to the age-day theory, but claims that this meaning of 'day' is 'philologically unsound'.<sup>85</sup> He refers to the gap theory,<sup>86</sup> but denies this on the basis that in Genesis chapter two the heavens and earth were said to be created

in one day. He emphasizes (and misunderstands) the supposed differences between the various schools of geology, and himself disbelieves the association of age and fossils. Any evidence for repeated cataclysms is local, the strata themselves are the evidence and remains of the universal flood. He has then, of course, to deal with objections (made by Fleming in the 1820's to Buckland's theory) that Scripture indicates no major change in landforms during the flood, and suggests that 'it would appear that the region near which the ark floated was in a great measure exempt from the violent effects of the convulsion with which the earth had been shaken to its centre ...'<sup>87</sup> Anyway, with the useful information that the 'week of Creation ... occurred between Sunday October 23rd and Saturday the 29th BC 4004' it may be best to leave Nolan's theory.

The British Critic (as we shall see again below) well summarized the strange nature of Nolan's book:

'In short, of Dr Nolan's volume we scarcely know what to say. His head seems to contain more than he knows exactly how to manage. His lectures are learned, but, unfortunately, they are almost unreadable; they display large stores of various information, but that information seems ill assorted, and sometimes almost obsolete; and his views are delivered, for the most part, in a style so pedantic and elaborate, that it rather tends to confuse the mind with a conglomeration of words, than to convey to it any notions clear and precise, and accurately defined. If we look to some single sentences, we find the vicar of Prittlewell no more an advocate for a literal interpretation of the Mosaic record than the Savilian professor himself: but then, unless he is an advocate for



such an interpretation, the general tenor of his discourse becomes altogether vague and unintelligible.'<sup>88</sup>

"Nolan, for all his confusion, is not anti-science. He speaks in terms of glowing respect for Newton, Boyle, Ray, and other scientists and their work:

'By the constant survey of the objects of Nature in which the scientific are engaged; by the patient investigation of the laws by which its operations are conducted, our conceptions must be enlarged and our admiration heightened of the power and wisdom by which the system has been combined...'<sup>89</sup>

He insists, however, that unless we look above the secondary to the great first cause, then science is 'baseless and hollow'. So 'the immortal founders of experimental philosophy' recognised this. Nolan, then, wishes neither to reject the miraculous in the name of science, nor to reject science in favour of revelation. He has no objection to 'natural' explanations, for God acts in both natural and supernatural agencies:

'It may be, indeed, questioned whether the power and wisdom of the Creator is not more apparent in importing to the machinery of nature a self-acting principle which fits it for executing his purposes, than in exerting his continued agency in perpetuating its motion, and aiding its operations.'<sup>90</sup>

Nolan is not hostile to science, nor, indeed, to geology, for he (for example) clearly admired Greenough and referred to the 'able President of the Geological Society.'<sup>91</sup> Some of his statements could, admittedly, be read as though he would like to put constraints on the way in which geological study is pursued, though in truth

he is more concerned that independently reached theories of geology be accepted only if consistent with Scripture, than with actually (or supposedly) doing the geology with, as it were, Bible in hand.

We have noted for several of the Scriptural Geologists that their views, even when accompanied by a Biblical Literalism, were no guarantee of Evangelical orthodoxy. Often they differed from the Evangelical leaders on other issues, and this is surely the case with the bellicose Henry Cole in his book in 1834. He linked the evils of geology with the 'tragedy' of Catholic Emancipation in 1829. Yet the Emancipation of the 1829 Act had the support of virtually all Evangelical leaders. In this issue Sedgwick was a more 'orthodox' Evangelical than Cole. Cole's book is churlish and obscurantist; his book is vituperative and offensive and makes no attempt to explain the findings of geologists conformably to Genesis (excusing himself from the task on grounds of ignorance of geology). In this sense he was atypical even of so called 'Scriptural Geologists', and it is not surprising that the Christian Observer preferred to be vituperated by him (along with men of the spiritual and intellectual calibre of Sedgwick) rather than show him any sympathy.

In the 1838 BAAS meeting at Newcastle Sedgwick had no office, though it was on this occasion that he preached a famous outdoor sermon on the sea-beach at Tynemouth to three or four thousand colliers, workmen and some employers, as well as delivering a concluding address to the BAAS of 'eloquence, astonishing beauty and grandeur'<sup>92</sup>. It was, however, in this year that the Rev. George Young D.D. communicated in Abstract to the Geological Section of the BAAS his 'Essay' on Scriptural Geology. But again the author (who had also written a 'Geological Survey on the Yorkshire Coast') was not anti-science, or even anti-BAAS. He did not reject the

existence of strata, but claimed (as others) that the secondary and tertiary rocks were formed by the deluge.

Sedgwick appears to have had no direct contact with Young, but in 1838 there also began the dealings with one whose system was similar (who indeed cites Young). This was the Dean of York, William Cockburn, who did have direct communication with Sedgwick. Like some others placed in this section amongst the 'Evangelicals' his Evangelical orthodoxy might be questioned. In his remarks (1842) on infant baptism he takes a more or less Evangelical line<sup>93</sup>, but his early background casts some doubt on this. In 1805 he claimed to have shown that 'extemporaneous public prayer by one person is absurd,'<sup>94</sup> and in 1807 calls Catholics 'Christian brethren' and says that if those in England would 'inform us more exactly of their present articles of faith, I flatter myself, it would be found that the two Churches differ much less than is supposed.'<sup>95</sup> On both these issues Sedgwick was nearer than Cockburn to the Evangelical leaders of the early nineteenth century. If we question his Evangelical orthodoxy we might equally question his importance. Unlike, say, Chalmers (whose authority was ascendant and works widely respected) or J.B. Sumner (then a widely respected Bishop heading eventually for Canterbury), Cockburn (for all his family connections) was a nonentity who would probably have continued in a well deserved obscurity but for his controversy with the members of the BAAS.

Cockburn did, however, thrive on controversy, and his objections on the science issue may be summarised in two points:

- (a) The geologists have got their facts right but their interpretations wrong
- (b) The BAAS is not a suitable arena for scientific discussion and dissemination.

If we begin with the first of these points, it may be useful to

- (i) Remonstrance to the Duke of Northumberland Upon the Dangers of Peripatetic Philosophy (1838)
- (ii) Letter to Professor Buckland (1838)
- (iii) The Creation of the World (To Murchison, 1838)
- (iv) The Bible Defended Against the British Association (1845)
- (v) A New System of Geology (1849)

At all times Cockburn accepts their geological researches, usually notably respectfully. Thus in (i) (which spends much of its time attacking the undulatory theory of light) he refers to 'geological facts elicited by modern science'.<sup>96</sup> In the same year he writes to Buckland (ii) 'With respect to geological facts concerning which we are about to reason, I take them from your book ....'<sup>97</sup> and he repeats almost verbatim his point in (i): 'every fact made known to us by geologists may fairly be explained by minute attention to the Mosaic history'. In (iii) he waxes deferential to Murchison:

'Too much praise cannot be given to the perseverance and assiduity with which you have investigated the facts connected with the subject. But while, with ready deference to superior experience, I presume not to doubt any of those facts, I feel myself entitled to affirm that you have offered no rational or satisfactory account of the probable origin of the various formations which you have so well described.'<sup>98</sup>

In (iv) (printed with letters and appendices) Cockburn does undertake to defend himself against the charge of total ignorance made against him by Sedgwick and others. He claims to have been 'constantly employed for many years in examining all the accessible strata in Devonshire and Yorkshire', to have inspected stone quarries and to have walked through 'all the tunnels of the railroad

between Bath and Bristol.'<sup>99</sup> Of the 'principal facts made known by the Geologists' there is 'no dispute',<sup>100</sup> He repeats this again, and asserts that he feels that all these facts are best accounted for by a 'literal interpretation of the Mosaic history'.<sup>101</sup> He feels, also, that the geologists (Sedgwick, Murchison, Buckland, etc.) have closed ranks in refusing to debate seriously with him (though Sedgwick sent him two long polite letters by his own admission.<sup>102</sup>)

Article (iv) was the printed version of an address delivered to the BAAS in 1844 when it met at York. Because of the Dean's standing he had been given leave to present his paper to the geological section, and it was Sedgwick who replied to it. A report of the speech in Chambers Edinburgh Journal <sup>103</sup> says Sedgwick spoke for an hour and a half, charming his audience with his vast learning. Clark and Hughes add that, according to an eye witness, the speech was delivered with a 'scornful bitterness', and also add that, at that time, (and Clark and Hughes were writing in 1890, nearly half a century later) '(the Dean) represented the feelings of a large majority of his countrymen.' They base this on the number of editions the pamphlets went through. It does need to be remarked, in view of the myths propounded even more recently about the strength of 'literalism' at the time, that this is a very poor way to judge a theory. Chambers' anonymous Vestiges of the Natural History of Creation was published in that very same year, and also went through numerous editions - but this did not prove that its readers agreed with its sentiments, any more than with Dean Cockburn's.

The speech of Sedgwick as reported in the Athenaeum (5th October 1844) contains comment on a number of issues. On the actual details

of geological knowledge, of course, Sedgwick could make mincemeat of the good Dean. The Dean's objections to orthodox geology's interpretation of the strata are answered, and some of the difficulties and absurdities of the Dean's own system are exposed. But Sedgwick's comments also referred to the purpose of the BAAS:

'Our object is, by the comparison of facts; the sifting of them, by kindred spirits meeting together, in the pure love of truth, for the advancement of science, and thus ascending to higher generalisations, and the knowledge of those laws by which individual phenomena are governed ... As we advance in the discoveries of science, facts multiply so fast upon us that they would become unmanageable, if we could not group them by certain resemblances, or include them under some simple law, which is merely an expression of a general conclusion derived from facts which we know to be true, and from which all phenomena proceed as necessary and inevitable consequences. The moment we arrive at the knowledge of such a law, we can assume, in a certain sense, a prophetic character, and predict events with certainty, because we know that the Author of nature is unchanging in his operations, and that the same effects will follow the same causes in times to come as in times past ... the primary object of this Association ... is the furtherance of physical science, on the principles pointed out. On this ground I hold it as certain, that the discussion of broad theoretical questions and cosmogonies, like those now brought before us, is utterly unfit for the present meeting ... We have nothing to do, as members of the Association, with moral or religious or political truths, in which elements of human passion are so liable to be mingled.'

A number of interesting points are raised here. First, there is the nature of scientific methodology, a topic to which we shall return in the next section. Sedgwick speaks of a 'general law' which is 'derived from the facts'. Presumably here he is thinking of the age of strata as 'derivable' from features of them (crinoids in living place etc.). Yet he shows no idea of the hypothetico-deductive methods of Whewell and Herschel, let alone the idea of 'theory-laden' observation implicit in Whewell. Moreover, Sedgwick himself, in 1829, had distinguished 'speculative geology' from 'the facts' much as the Dean did in 1844, though Sedgwick thought Ure not the man to construct it. Perhaps Sedgwick was meaning to restrict it to theories like those of e.g. Elie de Beaumont to explain the pattern of the mountain ranges. However his discussion as reported by the Athenaeum shows little sophistication on the level of methodology, and slurs over an essential similarity between the Dean and Sedgwick himself in the distinction between 'facts' and 'theory'.

The second point is that, though Sedgwick's faith in the possibility of science at all is linked to his faith in the consistency of God, he assumes that moral and social issues have no place. This is strange in view of the emphasis on natural theology which existed in many Presidential addresses. He also assumes that the BAAS is for the furtherance of physical science, but is vague as to how the papers and discussion do this.

Indeed, the nature and purpose of the Association was another point about which Cockburn, and a considerable

section of High Churchmen, had doubts. Cockburn had definite views about education and science. In a Sermon on the Evils of Education Without a Religious Basis (29th Sept. 1844) he asked the relevant question:

'What is the use of knowledge ? I mean the knowledge of earthly things. If it expands the intellect, and leads the mind more thoroughly and readily to appreciate the value and importance of Heavenly things, then it is indeed, as Solomon says, better than rubies. But if it begins and ends - and means to begin and end in mere mundane pursuits - it is likely to produce more evil than good.'<sup>104</sup>

He feels that a 'little progress in science, a little pre-eminence in literature fills the mind with a lofty notion of its own superiority'.<sup>105</sup> This is why 'so many men of learning and deep thought have been opposed to Christianity'. It is not that they have considered the evidence and rationally decided against Christ, rather 'it was pride - human pride - the pride of superior talents that made them refuse to consider proofs.'<sup>106</sup> Cockburn is not against knowledge, but he fears that it can lead to pride, which in turn leads to a rejection of true wisdom.

This applies to his strictures on the BAAS and the geologists. He is not against science as such, nor does he think it improper in a university:

'If a person who devotes his thoughts to scientific investigation, make any discovery of supposed importance -



or if he find some new proof of any doubtful proposition, it is very desirable that he should communicate his success to the studious world by printed explanations, so that his discovery may be considered and reflected on by those engaged in similar investigations, or he may with great advantage discuss such subjects at one of our universities, or in any place where the sons of science are permanently resident.<sup>107</sup>

The BAAS was not, in his view, a suitable setting to discuss scientific truth, it was too theatrical and engendered the kind of human pride he thought could lead to rejection both of evidence and of God. But he was not anti-science as such.

What should we think, then of the assertions by Derek Orange:

'Cockburn's more fundamental objection was to science as an institution, and in this he was supported by some of the new and controversial voices from Oxford.

If it was the religiousness of the British Association which alarmed the Dean ....<sup>108</sup>

Dean Cockburn did not object to science as an institution, rather he thought the theatrical nature of the BAAS improper to its conduct.

This chapter has given only a summary of some of the issues, and some more details, and many more references to books on either side of the 'Scriptural Geology' controversies may be found in an article by Millhauser.<sup>109</sup> As usual, however, Millhauser obscures the position of moderate Evangelicals. Thus Chalmers' gap theory:

'Chalmers' suggestion was favourably received by theological

liberals, and encouraged the rise of a more or less conscious party of "reconciliation."<sup>110</sup>

Sumner, amongst those quoted, is not usually called a 'theological liberal'. Moreover, Millhauser notes the connection of the Record with Scriptural Geology, but studiously ignores the position of the Christian Observer which was indisputably in the period the mouthpiece of the moderate Evangelical leadership.<sup>111</sup> Again, it may be argued that Millhauser exaggerates the actual power of the 'Scriptural Geology party'. Since the Scriptural Geologists often differed over their own pet schemes, it is quite difficult to think of them exactly as a 'party' - and in any case their actual power in the hierarchy of the Church is very much in doubt.

We began this section (6) (in part 6.2.1) citing the view of some modern scholars that natural theology in the early BAAS might be used to defend the Association against religious critics. We shall consider this in the light of all the critics in chapter 6.4 below, but at this point it may be useful to summarise some points relevant to the criticisms made by the Scriptural Geologists of the members of the BAAS:

- (a) Very few even of Scriptural Geologists were critical of science as such. Their criticisms might be:
  - (i) of scientific education if not included in a broader spiritual teaching and context.
  - (ii) of specific aspects of geological theory (not, they claimed, of geological observation) which appeared to contradict a literal interpretation of Genesis.
- (b) Even moderate Evangelicals were aware that natural

religion could be either 'an important step in the evidences for Divine Revelation' or 'insidious and fatal as a substitute for it'. In itself it was no guarantee that a person was a Christian rather than a Deist or Pantheist.

- (c) The nature of BAAS (according to Sedgwick) precluded moral and religious questions - and so could not include reaffirmations of a belief in certain interpretations of Scripture. To affirm natural religion without such affirmations could be mistaken for substitution.

Natural theology, then, even including the geologists' proofs of creation in time, could easily be accompaniments to Deism. The Scriptural geologists, for all their differences and confusions, are all concerned in their different ways not merely with vague piety but with loyalty to (as they saw it) the actual words of Scripture. The vague affirmations of natural theology could do little to allay suspicions of unorthodoxy on Genesis amongst those geologists of the BAAS who might (like Sedgwick) also be Anglican clerics. In actual fact, such affirmations might rather increase suspicions that such men were really crypto-deists, since (by the nature of the BAAS) these affirmations could not be accompanied by specific theological statements.

## 6.2 Notes

1. J.H. Brooke, 'Natural Theology and the Plurality of Worlds', Annals of Science, 1977, p. 252.
2. A. Thackray and J.B. Morrell, Gentlemen of Science, p. 224.
3. Ibid., p. 228.
4. Ibid., p. 230.
5. Ibid., p. 234.
6. Ibid., p. 235.
7. Ibid., p. 236.
8. Thackray and Morrell state: 'Going further than any other liberal Anglican divine had dared, [Powell] suggested that Genesis could not be regarded as historic narrative.' (Ibid., p. 239).
9. Ibid., p. 237.
10. C.A. Russell, Science and Social Change, 1700-1900, chapter 4.
11. H. Gunning, Reminiscences of the University Town and County of Cambridge, 1, p. 236.
12. Clark & Hughes, 1, pp. 132 & 162.
13. W. Carus, Memoirs of the Life of the Rev Charles Simeon, p. 280.
14. See e.g. ibid., p. 311f., Winstanley, Early Victorian Cambridge, p. 20f.
15. British Critic, 1831, 9, 71-90.
16. Third Report of the B.A.A.S., 1833, p. xxvii.
17. See e.g. his letter (1823) to the Duchess of Beaufort, Carus (ref. 13), pp. 582-8.
18. Letter to J.W. Cunningham (1821), Carus (ref. 13), pp. 552-4.

19. The letter is in the Whewell Trinity Collection, Add Ms a 78/30.
20. Carus (ref. 13), p. 722.
21. Phillipians 3.7-8: 'But what things were gain to me, those I counted loss for Christ. Yea, doubtless, and I count all things but loss for the excellency of the knowledge of Christ Jesus my Lord: for whom I have suffered the loss of all things, and do count them but dung, that I may win Christ, and be found in him...' (King James Version).
22. Romans 3.1-2.
23. This is shown by the continuation of the quotation in note 21, where 'knowledge' is equated to being found in Christ.
24. W. A. Brown, Recollections of the Conversation Parties of the Rev Charles Simeon, pp. 325-6.
25. His biographer, Canon H.E.Hopkins, has informed me that in all Simeon's years of preaching he seems to have dealt only with the creation of man and not with that of the world.
26. Carus, 'Reminiscences of Professor Sedgwick', The Churchman, 1889, 3, 225-237. On p. 225, Carus refers to Sedgwick's 'brilliant series of remarkable lectures on geology'. It might be argued that in retrospect at so much later a date, Sedgwick's lectures appeared less unorthodox than at the time. But if we judge simply by the actual evidence of what Carus said, then he accepted their value.
27. Gorham wrote in a letter to his father that St Johns voted against him 'as being a Methodist' (Clark & Hughes, 1, p. 158). Gorham was, of course, an Anglican, but 'Methodist' at that time was a term often applied to Evangelical Anglicans.

28. On 17th May 1818 he wrote to his father 'Some few (like our dear friend Farish) were taken in by anticipation....' (Clark & Hughes, 1, p. 157); but it does not seem to have occurred to him that Farish might feel it inappropriate for an Evangelical to seek the Professorship. Gorham, according to Clark and Hughes, was before Sedgwick in issuing a manifesto promising to lecture.
29. Clark & Hughes, 1, pp. 157 & 161.
30. Clark & Hughes, 1, pp. 157 & 160. Pryme, though a friend of Sedgwick, voted for Gorham because Gorham had previously studied geology.
31. Winstanley, Early Victorian Cambridge, p. 31.
32. Clark & Hughes, 2, p. 349; Sedgwick annotated a copy of Clarke's syllabus during the lectures (Clark & Hughes, 1, p. 162).
33. Carus, (ref. 13), p. 311.
34. Canon, Science in Culture, p. 33.
35. See e.g. A.R.Hall, The Cambridge Philosophical Society. Farish was the first President, Lee and Sedgwick were secretaries, when the first meeting was held on December 13th 1819. The objectives were stated: 'That this Society be instituted for the purpose of promoting scientific inquiries, and of facilitating the communication of facts connected with the advancement of philosophy and natural history.'
36. Little material is available on Henslow, but the Memoir of the Rev. John Stevens Henslow, by L. Jenyns (1862) gives some insight. Henslow belonged to no church party (p. 132), and: 'He rather seemed to take his stand upon the Bible itself, which he daily studied, and with which he was thoroughly acquainted. The groundwork of his ministerial teaching was in strict accordance

with the Sacred Volume, as it was also with the Articles of our Church...' During his final illness he 'expressed the most sincere gratitude to the Almighty for His mercies to himself, and placed his entire trust in the Saviour, with absolute renunciation of all personal merit.' (p. 258). After Sedgwick's final visit the latter remarked that he 'never saw and conversed with a human being whose soul was nearer heaven.' (p. 262 and also Clark & Hughes, 2, p. 371).

37. J.S. Reynolds, The Evangelicals at Oxford 1735-1871, p. 110.
38. F.S. Taylor, 'The Teaching of Science at Oxford During the Nineteenth Century', Annals of Science, 1952, 8, 82-112.
39. W. Tuckwell, Pre-Tractarian Oxford, p. 43.
40. Canon (ref. 34), p. 36.
41. Buckland, Geology and Mineralogy Considered with reference to Natural Theology, p. 22.
42. Ibid., p. 33.
43. The Life and Correspondence of William Buckland, Ed. E.O.Gordon, p. 105.
44. Ibid., p. 111.
45. A general schema of possible views is given above on p. 263, and Simeon's open view on the 'gap theory' has already been cited on pp. 273-4.
46. See J.D. Yule, The Impact of British Religious Thought in the Second Quarter of the Nineteenth Century, ch. 9.
47. Christian Observer, 1815, pp. 246-248.
48. Christian Observer, 1832, p. 692.
49. Christian Observer, 1832, pp. 742-4.

50. Christian Observer, 1833, p. 219.
51. It did, however, carry an article on his petition to the House of Lords on Ecclesiastical Improvements from Sedgwick's Evangelical friend Canon C.N. Wodehouse. (p. 561).
52. Christian Observer, 1834, pp. 199-207.
53. Christian Observer, 1834, pp. 306-309.
54. Ibid., pp. 308-9.
55. Ibid., p. 309.
56. Ibid., pp. 309-10.
57. Ibid., pp. 310-16.
58. This position Sedgwick maintained consistently throughout his life, as late as 1865 signing the famous 'Scientists' Declaration' which stated as much. Daubeny, significantly, was highly critical of the whole approach of the Declaration. (See W.H.Brock Brock and R.M.Mcleod, 'The "Scientists' Declaration"...', BJHS, 1976, 2, 39-66.)
59. Christian Observer, 1834, p. 371.
60. Ibid., p. 370.
61. On p. 371 the Editorial makes a note to 'some of the theological opinions expressed in Sedgwick's otherwise splendid commencement sermon' which are 'not to be commended'. It starts the note 'We quote, after Mr.Cole the following specimens...' Sedgwick's irritation at Cole, but anxiety that more friendly and respected commentators not be misled by him, are noted above on p. 52 and notes 48 and 49 to chapter 2.3.
62. Ibid.



63. Christian Observer, 1834, pp. 449-51.
64. Ibid., p. 471.
65. See above, part 2.1.3.
66. Yule (ref. 46), states that 'the following of the Scriptural Geologists, for all their vociferousness and the plenitude of their tracts, was small and consistently so.' (p. 328).
67. See W.V. Farrar, 'Andrew Ure, F.R.S., and the Philosophy of Manufactures', Notes and Records of the Royal Society of London, 1972-3, 27, 299-323.
68. See ibid; reference is made to his 'combative and rancorous disposition' (p. 299), the Clarke visit is mentioned p. 305, but the 'intemperate polemics' noted on the next page.
69. Ibid., p. 301, citing Ure's Dictionary of Chemistry (1821) p. xv.
70. Ure became an honorary member of the Geological Society soon after its foundation in 1807 (ibid., p. 312), and an F.R.S. in 1822 (ibid., p. 305).
71. See ref. 16, p. 419.
72. The letter is in the University Museum Phillips collection in Oxford, dated 25th February, 1839.
73. In 1830 the geologist Robert Bakewell wrote concerning Ure's book on geology: 'Dr Ure is said not to be a practical religionist any more than he is a practical geologist...' (G.P. Fisher, The Life of Benjamin Silliman, 2, p. 51). His marriage ended in acrimony and divorce.
74. Proc. Geol. Soc. Lond., 1830, 1, p. 208.
75. Ure also adulated Newton and Paley.

76. Sedgwick (ref. 74), p. 210.
77. DNB, p. 1500.
78. Reynolds (ref. 37), p. 110.
79. Nolan, Objections of a Churchman to Uniting with the Bible Society.
80. Ibid., pp. 5-6.
81. Ibid., p. 50.
82. Clark & Hughes, 2, p. 585 refer to such a meeting when Robinson was an undergraduate in the later 1840's.
83. Nolan (ref. 79), p. 62.
84. Nolan, The Analogy of Revelation and Science, p. 38.
85. Ibid., p. 101.
86. Ibid., p. 102.
87. Ibid., p. 314.
88. British Critic, 1834, 30, p. 414.
89. Nolan (ref. 84), p. 1.
90. Ibid., p. 14.
91. Ibid., p. 454.
92. Cited in Clark & Hughes, 1, p. 515 from a letter of Herschel's.
93. Cockburn, Some Remarks Addressed to the Lord Bishop of London...
94. Cockburn, An Address to the Methodists, p. 17. Since this was written whilst Cockburn was Christian Advocate in the Cambridge University where Simeon's Evangelicalism was ascending, it is all the more extraordinary.
95. Cockburn, An Address to the Roman Catholics, p. 20n.

96. Cockburn, Remonstrance...., p. 10.
97. Cockburn, Letter to Professor Buckland, p. b.
98. Cockburn, The Creation of the World, p. 2.
99. Cockburn, The Bible Defended...., p. 16-17.
100. Ibid., p. 17.
101. Ibid., p. 21. In view of this it is puzzling to read in a recent book that Cockburn 'attacked the "reconcilers" of Genesis and geology, especially Adam Sedgwick and William Buckland...' (T. Cosslet (Ed.) Science and Religion in the Nineteenth Century, p. 17). Since Cockburn himself was also attempting to reconcile geological findings with Genesis, it could not have been the reconciliation as such to which he was objecting.
102. Cockburn, The Bible Defended...., p. 34.
103. Edinburgh Journal, 1844, 2, p. 322.
104. Cockburn, Sermon...., p. 5.
105. Ibid., p. 7.
106. Ibid., p. 8.
107. Cockburn, Remonstrance...., p. 5.
108. Orange in Parliament of Science (Ed. Mcleod and Collins) p. 58.
109. Millhauser, 'The Scriptural Geologists...', Osiris, 11, 65-8.
110. Ibid., p. 67.
111. The Record is noted (p. 74) for its attack on Sedgwick. On p. 75 note 21 Millhauser states: 'After 1832 the London Christian Observer opened its pages to clergymen supporting reconciliation; Conybeare contributed to it in this vein in April, May, June and August 1834.' But he does not indicate that the editorial

supported reconciliation both before and after 1832, nor would one gather from the article the respective positions of the Christian Observer and Record in the Evangelical world.

There seems to be a tendency of some scholars to call anyone who was not a bigot a 'liberal'. Poor Chalmers and Sumner (both leading Evangelicals) often seem to come in for this kind of treatment. Millhauser also calls John Pye Smith a 'liberal clergyman' (p. 77) because in the late 1830's he reached conclusions of a local flood and local re-creation, and published lectures and works on this. John Pye Smith was perhaps the leading Congregationalist of his period, and was not a bigot. He corresponded with Phillips on the geology and Genesis question (these letters are in the Oxford University Museum Phillips collection). But it is questionable whether a 'liberal clergyman' is a very accurate description of him.

### 6.3 Mid and High Churchmen and Science

#### 6.3.1 The Christian Remembrancer

We have noted (in part 2.1.3) the rather 'High' viewpoint of this publication in the early 1830's. What may interest us is its attitude to (i) natural theology, (ii) Scriptural Geology, and (iii) BAAS. Would Sedgwick and his circle have found any potential threat from it?

On natural theology it reviewed Whewell's Bridgewater Treatise, an 'admirable treatise', and commented 'Mr Whewell has performed the task assigned him with no less credit to himself than advantage to the reader.'<sup>1</sup> Its review of Chalmers' Bridgewater in the same year makes no criticism of the natural theology as such.<sup>2</sup>

On Scriptural Geology its line is similar to the Christian Observer. In 1831, for example, it refers to Lyell, Buckland and Sedgwick: 'whose talents and researches all who have read their writings must acknowledge.'<sup>3</sup> It reviews Scriptural Geologist H. Browne in April 1833, with a bitingly sarcastic review, calling him 'deplorably ignorant'.<sup>4</sup> In July 1833 it carries two further reviews of Scriptural Geologists W.M. Higgins and George Fairholme (whose books were published in 1832 and 1833), depicting Scriptural Geologists in general as dabblers forcing on the world 'a mass of crude illogical rhodomontade'.<sup>5</sup> Compared with the Remembrancer Sedgwick's comments on Scriptural Geologists seem positively polite! Not that, of course, it saw objective corroboration for Scripture as valueless - for in 1832 it recommended the Evangelical Bishop Wilson's book on the evidences.<sup>6</sup> But, like the Christian Observer, it supported orthodox geology and could not support those whose analyses it saw as based on ignorance - however laudable their motives.

The BAAS is first mentioned in 1833<sup>7</sup> with an extract from its rules without comment. Later in 1833 it commented: 'The first meeting was held at York in 1831; Oxford next had the honour of receiving within

its walls the many distinguished characters of the Association, and Cambridge has now witnessed a combination of talent never before seen within its precincts.<sup>8</sup>

On natural theology, Scriptural Geology, and the BAAS, the Christian Remembrancer was totally supportive of the Sedgwick circle.

### 6.3.2 The British Critic Pre-1837

On natural theology, the British Critic reviewed Whewell's Bridgewater in July 1833 with glowing praise, calling Whewell a 'mighty master' of 'conspicuous talent'.<sup>9</sup> The argument from design is applauded<sup>10</sup>, though Paley's ethical system is rejected in favour of Butler's.<sup>11</sup> In October 1833 Chalmers' Bridgewater is reviewed with only marginally less enthusiasm, and the British Critic strongly defends the value of natural theology in its proper place - in terms not at all dissimilar from those of Sedgwick himself.<sup>12</sup> The review also accepts a mind-matter dualism as 'sanctioned by the instant and well nigh universal consent of mankind' - another similarity to Sedgwick. In July 1835 Brougham's Discourse, and in July 1836 Turton's comments on it are reviewed.<sup>13</sup> Though critical on some aspects (Brougham's lack of emphasis on both conscience and revelation), the review is positive - finding in Brougham an unexpected ally. Then, in 1836, Buckland's Bridgewater is reviewed with glowing praise, Buckland's name stands 'among the very first' in his science, and this will enlarge his reputation.<sup>14</sup> Finally, we may note that Sedgwick's own Discourse was reviewed in 1834.<sup>15</sup> It's opinion of both the person and talents of Sedgwick is high, and it approves both the tone and content of the Discourse. Its main criticism of Sedgwick (which colours the whole review) is that he has departed from the logic of his own Discourse in supporting the admission of the Dissenters to the University.<sup>16</sup>

On Geology, we note that in January 1831 in context of the declinist debate, the achievements of both Buckland and Sedgwick are praised,

Sedgwick being 'acknowledged by all to be as distinguished in philosophical views as in activity of observation...'<sup>17</sup> The British Critic 'rejoiced' at the appearance in 1831 of Lyell's Principles, with a perceptive view on the work's role and with reference also to the 'excellent little work' by Conybeare and Phillips.<sup>18</sup> A review of Sharon Turner's work in 1832 brought some general comment on science and revelation.<sup>19</sup> Any apparent discrepancy between the two, it said, must be based on misunderstanding - though there are dangers in linking revelation too closely to a science which might later change. The Christian philosopher should not 'direct his steps in the pursuit of physical truth by a continual reference to the cosmogony of Moses...'<sup>20</sup> The review is critical both of Turner and Ure, and accepts an 'age-day' theory. A later review of Lyell, in April 1834, makes clear the British Critic's acceptance of the not 'strictly philosophical' language of Genesis.<sup>21</sup> This particular review is rather more muddled in its actual geology, and seems to be critical both of the age-day and the gap theories.

Later in 1834 the British Critic carried an interesting review of the Nolan-Powell debate.<sup>22</sup> Nolan's book is confused (it says) and in fact he is he is no more 'literalist' than his opponents. The Bible uses popular language, not meant to teach science. On the other hand, it asserts that where the Bible does describe events they actually did happen. As for geology, it is too early yet to erect a system. Of Powell, then, he 'is inclined to throw overboard not merely the verbal and literal accuracy of the scriptural narrative, but its general and substantial truth in any intelligible sense of the word.'<sup>23</sup> Thus 'We do not object to his cosmological researches, but we enter our decided protest against his theological crudities.'<sup>24</sup> Like the Christian Observer, it sees differences between Powell and Sedgwick: 'There may be some sympathy between Mr. Powell and Mr. Sedgwick; but we should be sorry to think that

the views of the two professors were identical.<sup>25</sup> The British Critic, like Sedgwick, though ultimately revelation and geology would harmonise, but that insufficient knowledge was then available to do this. It added, however, that Powell was mistaken in supposing either that it saw the BAAS as a threat or that scientists need fear persecution.<sup>26</sup>

Finally, we may note that in its review of Buckland's 1836 Bridgewater the British Critic, noting Pusey's support for 'his friend', finds 'highly satisfactory' the version of the gap theory which he proposes.<sup>27</sup>

#### 6.3.4 The British Critic, Newman and the BAAS

During 1838 the British Critic came under the influence of Newman and his circle. In January 1839 it carried a review of the BAAS, which has been referred to by modern scholars such as Derek Orange as part of a 'thunder of the critics' of the BAAS.<sup>28</sup> It is important, however, to see wherein precisely the criticisms lay.

The reviewer denies that he deprecates that 'glorious path to knowledge' opened by the physical universe, and is friendly to science as a valued part of knowledge.<sup>29</sup> He also denies any alarm 'when the geologist talks of a virtual infinity of time in our planet's history.'<sup>30</sup> Scientists need not be timid or fear discovering too much<sup>31</sup>, and he recognises their need to meet to discuss and impart discoveries.<sup>32</sup>

He does, however, object to what I have called the razzmatazz, seeing it as detrimental to the serious purpose of the BAAS<sup>33</sup> though thinking that it will in any case disappear with age.<sup>34</sup> More seriously, though, he feels that Christians should pursue science in a context of their relationship with God through Christ, whereas in the BAAS there was a danger that scientific fellowship might replace Christian<sup>35</sup> and a religion of 'natural science'<sup>36</sup> be adopted in Universities where Dissenters were admitted on the basis of a 'glorious brotherhood' of science.<sup>37</sup> I will discuss later whether natural theology might allay such fears.



6.3 Notes

1. Christian Remembrancer, 1833, 15, pp. 257-261.
2. Ibid., p. 402.
3. Christian Remembrancer, 1831, 13, p. 666.
4. Christian Remembrancer, 1833, 15, p. 205.
5. Ibid., p. 391.
6. Christian Remembrancer, 1832, 14, p. 413.
7. Christian Remembrancer, 1833, 15, p. 256.
8. Ibid., p. 509.
9. British Critic, 1833, 27, p. 101, p. 97.
10. Ibid., p. 78.
11. Ibid., p. 83.
12. British Critic, 1833, 28, pp. 239-282, see p. 251.
13. British Critic, 1835, 35, pp. 210-224, 1836, 30, pp. 27-40.
14. British Critic, 1836, 40, pp. 295-328, see p. 296.
15. British Critic, 31, pp. 186-199; see also part 2.3.6 above.
16. See above, part 2.3.6.
17. British Critic, 1831, 17, pp. 71-90.
18. Ibid., pp. 150-206, see p. 184.
19. British Critic, 1832, 23, pp. 64-88.
20. Ibid., p. 66.
21. British Critic, 1834, 30, p. 354.
22. Ibid., pp. 411-434.
23. Ibid., p. 415.
24. Ibid., p. 432.

25. Ibid., p. 416.
26. Ibid., p. 433. The British Critic explicitly stated that 'We accuse not the British Association of any conspiracy against the religion of Christendom,' and it reaffirmed the freedom of the members of BAAS to scale the heights of science or invention without molestation. Powell, it implies, need have no persecution complex - he will not become another Galileo!
27. British Critic, 1836, 40, p. 300.
28. See below, part 6.4.1.
29. British Critic, 1839, 49, pp. 8-9, 12.
30. Ibid., p. 13.
31. Ibid., p. 14. William Smith is worthy of praise (p. 26).
32. Ibid., p. 14.
33. He speaks of the dilittanti, the bustle and publicity, and the sensationalism (Ibid., pp. 16-28). The Newman circle were not very familiar with the ways of science (Sedgwick actually wrote in his fifth Discourse that Newman was totally ignorant of science) and seem to have imagined that the important part of a scientific conference was in the formal sessions rather than the informal contacts made.
34. British Critic, 1839, 49, p. 46.
35. Ibid., p. 19.
36. Ibid., p. 41.
37. Ibid., p. 43.

## 6.4 Natural Theology in the Early British Association

### 6.4.1 Natural Theology and the Defence of the BAAS

We come now to assess the questions asked at the beginning of this section (6), concerning the role of natural theology in the early BAAS, and in particular in the thinking of Sedgwick as a leading early member of it.

The Thackray-Morrell thesis that natural theology could be used to defend the Association against the religious critics does not seem very likely. Against which critics and criticisms might it prove effective ?

As we have seen, various criticisms were made against the BAAS. Surely, however, writers like A.D. Orange make these criticisms seem more unified and orchestrated than they really were ? In considering this it is worth quoting in full a passage from Orange's recent contribution to The Parliament of Science; he wrote:

'The sensitiveness of nineteenth-century Churchmen to scientific advance has usually been exhibited in terms of their disapproval of some of the propositions put forward by the scientists. But Cockburn's more fundamental objection was to science as an institution, and in this he was supported by some of the new and controversial voices from Oxford. If it was the irreligiousness of the British Association which alarmed the Dean, it was its pseudo-religiousness which incensed the Tractarians. In a generation much engaged with natural theology, the leaders of the Association were given to embellishing its public assemblies with many expressions of fulsome homage to God; not to the Christian God of the Church's articles and creeds, but to 'that fashionable idol of the present time', the Author of Nature. Indeed, the

British Association most palpably and vocally represented what the Oxford saints saw as the secularizing, extravagantly liberal mood of the day: it was only a short step from claiming that in the cultivation of science, religious differences could be put on one side to implying that religious convictions did not matter. The British Critic, the organ of the Tractarians, went on to the charge that there were many in the Association who would do away with the religious tests which, somewhat precariously, preserved the Anglican ethos of the ancient universities. And indeed there were: Adam Sedgwick of Cambridge, and Baden Powell of Oxford, with the active support of Babbage, Airy and lesser scientific lights such as James Heywood of Manchester, all prominently identified with the British Association, had in 1834 been instrumental in representations to Parliament on the admission of Dissenters to the universities. To the traditionally minded it was all of a piece with the conferring of honorary doctorates on Brewster, Dalton, Faraday, and Brown (not one of them a member of the Established Church) during the Association's visit to Oxford two years earlier.<sup>1</sup>

His earlier article, which has had more time to influence other historians of science, was even plainer. In this<sup>2</sup> he headed his section 2 'the Thunder of the Critics' and stated: 'the pattern of the future hostility began to appear when The Times devoted a leading article ...etc'. The Times, Nolan, Cockburn and Bowden are somehow welded into a unified opposition - with comments taken between 1832 and 1839, a period of seven years. The criticisms which Orange describes were certainly made, but there was no

orchestration, and moderate opinion in the early 1830's - both Evangelical and High Church - did not attack the BAAS. Orange later states: 'the early salvoes from Nolan and The Times convinced [the BAAS] officers that they must now justify the ways of science to men.'<sup>3</sup> This seems unlikely. Nolan did not attack science as such, only its conclusions in geology. His views even on this were not shared by either the Christian Observer or the British Critic - and those who did share his views would not have been mollified by natural theology or protestations of piety. The Times did not attack science as such, but the social nature of the BAAS. The Presidential protestations that science could lead to God rather than to atheism, where they did occur, were surely not intended to meet attacks from serious critics. I have not been able to find any serious critics - even including the Scriptural Geologists - who imagined such a thing necessary. But the popular mind may well be a different matter - and Presidential addresses were also designed for popular consumption. Natural theology, then, might have protected the BAAS against anti-science sentiment - but there is no indication that such sentiment played an important part in serious criticism. What real criticisms were made therefore?

One was on the specific theories of geology - as Dean Cockburn associated the 'unacceptable' geological theories both with Sedgwick and with the BAAS. But natural theology, far from blunting such criticism, served only to fuel it by suggesting to critics like Cole that natural religion was being set up in place of revealed. We have seen, moreover, that the threat on this score was not a serious one - both moderate Evangelicals and moderate High Churchmen supported orthodox geology. What seems to have been felt more, by Sedgwick at least, is the danger when such absurd 'flood

geologies' were actually introduced into the BAAS or Geological Society - e.g. by Ure or Cockburn. But, aside from scientific rebuttal, the BAAS insistence that religious issues be kept out of the affairs was a buttress against this. In actual fact, the introduction of natural theology into the proceedings - albeit only in the general comments of the Presidents and not in the actual sectional proceedings - might well have endangered this protective policy. And it certainly would not have prevented the critics from bringing their cases - for they were concerned with Bible based theology not with vaguely deistic pronouncements.

The second line of criticism which might have been feared concerned the showmanship and bustle of it all. Both Cockburn and Bowden made such criticisms. But expressions of piety would hardly have been likely to allay fears on this score. Presidential speeches and pronouncements usually added to the sense of 'occasion', and the large and showy gatherings which they entailed would have been seen as a hindrance rather than help to the quiet and sober scientific discussion which the critics thought endangered by the BAAS structure.

The third line of criticism, coming particularly from the Tractarians, concerned the interdenominational nature of the BAAS - associated with the advocacy of leading members like Sedgwick of the admission of Dissenters to Oxbridge. Again, far from blunting such criticism, the references to natural theology within BAAS served to fuel it. We have seen that the exaggerated language of some of its Presidents could easily be taken to imply that some kind of new more universal religion was to be set up, based on science and natural religion. Certainly Sedgwick and

probably most of the others did not really intend this or think it viable - but their erudition sometimes ran away with them. Had no reference been made to natural religion, criticism on this ground would have been much less likely. Whilst Newman and his associates might wish to preserve the Anglican monopoly in teaching and training institutions (as they viewed 'Oxbridge'), the BAAS was not for such purposes, but rather was for interchange of research information. There could be no serious objection to such interchanges occurring between men of very different religions - the danger came precisely when they sought to find common bases of theology. Had the BAAS stuck strictly to its concern for the advancement of science (exchanging research information in a sober and non-theological context) it would have been far safer from such criticism. We must remember that in this period other interdenominational bodies had to be very careful to avoid appearance of common worship. Even the Bible Society had no prayers in its meetings until 1857 because not all parties to it could be pleased. The BAAS would have been safer to leave out the religious aspect entirely.

This leaves just one possible 'criticism' which might have been 'blunted' by natural theology. It would be a religious claim that science was somehow of itself irreligious. We need to treat this possibility seriously, not least because in so many popular books a supposed 'science religion conflict' is regarded virtually as an inevitable fact of life. A respected historian can write of the 'Victorian conflict between science and Religion' and state: 'By the second quarter of the nineteenth century substantial developments in geology, physics, biology, physiological psychology, and philosophy of science challenged or cast into doubt theological assumptions and

portions of the Bible.'<sup>4</sup> But is a conflict so obvious a fact that we may focus on its facets rather than question its reality ?

It will be useful here to raise some more general points, with some suggestions for alternative perspectives in which to consider them, before focussing on the much more specific issues of the second quarter of the nineteenth century and the evidence already presented about it.

It must immediately be admitted that there are difficulties of adjustment for any belief system (scientific or religious) in the light of new facts or information. But I would like to suggest as a working hypothesis that the feeling abroad of the self evidence of a 'science religion conflict' arose largely after the conscious campaign of a group in the late nineteenth century committed to a 'positivistic' view of science. The third quarter of that century saw the rise of the X-club, with T.H.Huxley as one of its central figures, dedicated to eradicate the influence of religion on science. One of the group, Francis Galton, published in 1872 a work also purporting to show statistically a decline in clerical involvement in science.<sup>5</sup> Around the end of the period, two influential books by Draper and White<sup>6</sup> traced the supposed history of the religion science conflict.

Two distinct elements in this may be identified. Turner's article commented on the increasing 'professionalisation' in the period, both of science and of the clergy.<sup>7</sup> At the beginning of the century an Anglican clergyman was generally expected to perform little duty beyond ensuring that the required services were performed, and a curate could be kept to do that. The rise of Evangelicalism and then Tractarianism within the church brought more emphasis both on the care of souls and the dignity of the church. Thus by the third quarter it was becoming less acceptable for a clergyman to regard his



position as a mere sinecure. On the other hand, science was for various reasons becoming increasingly professionalised. Indeed the word 'science' had for some time meant a study of natural phenomena, but the word 'scientist' (first coined by Whewell) focussed on the person rather than the activity. Increasingly, then, the occupations of scientist and clergyman were expected to be 'full time' ones.

But division of labour is not necessarily the same thing as antagonism. In itself it proves neither that a 'religious' mind differs from a scientific one, nor that henceforth clergymen denigrated science and scientists were irreligious. Propaganda of positivists such as those of the X-club, and their modern counterparts,<sup>8</sup> may portray a rise of 'real science' as a struggle to throw off the trammels of religion and metaphysics. But this interpretation of history is not self evident. It *could*, for example, be claimed that earlier 'mystics' like Kepler and Newton might have found concepts like gravity harder to accept had they been positivists. In the nineteenth century, Darwin was religious when he first formulated his theory, and Wallace (to positivist embarrassment) stayed so until his death.<sup>9</sup> After the X-club's demise, it is arguable that the religion and metaphysics of figures like Poincare, Eddington, Einstein and Heisenberg enhanced their scientific vision and imagination. This theme will not be developed here, since it is of a marginal relevance to the subject of the thesis, but the point needs making. Huxley's vision of science as a kind of crusade, where no soldier could afford a divided allegiance, is highly questionable.<sup>10</sup>

Turner also quotes Huxley's famous remark: 'Extinguished theologians lie about the cradle of every science as the strangled snakes besides that of Hercules', but he adds 'But the history of science has been more complex and problematical.'<sup>11</sup> Turner notes

the rather less tidy truth, where science often goes astray, and scientific no less than religious communities are unreceptive to new ideas. But if Huxley could not be expected to be a good historian of science, was he at least right about his own period? Surely the period around the 1860 debate epitomises his assertion?

Two questions need to be asked: (i) were the critics of Darwin hostile to science as such? (ii) did theological opposition put Darwin's theory in any real danger of oblivion? The prima facie answer to (i) must be no. Neither Samuel Wilberforce, nor any other influential theologian suggested that the study of natural phenomena was wrong or harmful.<sup>12</sup> Yet certainly some would not like to see science free in the sense in which Huxley wanted. If one were to take the actual word 'science' to mean study of natural phenomena without metaphysical presuppositions (i.e. a supposedly positivist definition), then of course he was right. But to take it thus would be to exclude from 'science' most of which we now accept as epoch making in its history. Freedom from metaphysical assumption is not a realistic basis to declare a 'science-religion conflict'. But Huxley was also insisting that science be free and independent of biblical revelation. On this not all would have agreed with him, though Sedgwick (as a good Baconian) would have professed to do so. More recent experiences of 'Creationism' may be taken as illustrative of dangers at least to science education (if not to science itself), and of the general sterility of 'science' kept subservient to revelation.<sup>13</sup>

We cannot, of course, directly extrapolate back from American 'Fundamentalism' of the 1920's or 'Creationism' of the 1980's, to England in the 1860's. Yet a belief that Darwin's theory was under threat is certainly plausible. Men like Bishop Wilberforce were

well respected leaders in the Anglican hierarchy. Darwin, moreover, had presented and illustrated a plausible mechanism for evolution, but had little positive proof and many problems to overcome. The comments of critics like Wilberforce were not ignorant rantings, for Darwin himself admitted that Wilberforce's criticisms (in their written form) had real force.<sup>14</sup> There were, furthermore, a number of mainstream and well respected scientists who joined in the opposition to the theory, e.g. Sedgwick, Agassiz, Owen, and Jenkin, speaking in their own areas of competence. Many of the objections were undoubtedly scientific<sup>15</sup>, but Huxley had at least a prima facie case for believing that religious beliefs played a part in the motivation, even if a simple 'science vs religion' model is too naive.<sup>16</sup>

But if it were granted, with many caveats, to apply to the third quarter of the century and the controversies surrounding evolution by natural selection, there are great problems in extending similar ideas of conflict back to the second quarter. None of the Scriptural Geologists had the same church standing as Wilberforce.<sup>17</sup> Most were antiquarians, or historians, or at best (like Ure) were speaking on subjects considerably removed from their own fields. None seem to have been leaders in their scientific fields, as were some of Darwin's later critics. We have, moreover, seen at considerable length in the previous chapters, that they had little support from the mainstream and influential Christian journals.<sup>18</sup> For over thirty years their 'flood geology' type systems had been rejected by leading mainstream naturalists, many of whom were clergymen or men of undoubted personal piety. In short, though some Scriptural Geologists may well have wished to keep science in a sterile subservience to their interpretations of Biblical revelation, their chances of success appeared minimal whether the

members of the BAAS emphasized natural theology or not. What, then, are we to make of Turner's more general assertion that developments in geology, physics, biology, etc.etc.etc.were perceived to 'challenge or at least cast into doubt theological assumptions and portions of the Bible.' ? Presumably examples might be found from each area he cites where discoveries called into question someone or other's theological beliefs or interpretation of the Bible. But as a general characterisation of the period it is greatly distorted.<sup>19</sup> There is no evidence that in the second quarter of the nineteenth century either science felt seriously threatened by orthodox theology, or orthodox theology felt threatened by science. The general 'science religion conflict' in this period is a myth.

It is now time to return to comment on the question of the blunting of criticism by the use of natural theology. In this period not even the Scriptural Geologists suggested that science (i.e. the study of nature) was to be feared or avoided. No one suggested that study of science was an improper pursuit for a clergyman, provided it did not interfere with his religious duties. Since no such criticisms in general existed, natural theology could not have been emphasized to meet them. Any danger there might have been, of wanting to make science subservient to revelation, would hardly have been avoided by emphasizing natural theology even if the danger had been perceived as a serious one.

The conclusion must be that there seems no particular kind of criticism of science which the emphasis in the BAAS on natural theology could be designed to blunt. From such criticism as there was, an emphasis on natural theology was more likely to open the BAAS to attack as setting up natural in place of revealed religion. It would have been safer to say nothing.

#### 6.4.2 Natural Theology as a 'Mediating Influence' in the BAAS ?

There has been a second suggestion, that this natural theology acted as a 'mediating influence' between the members of different denominations in the BAAS. This, in fact, is a very difficult proposition to assess. Obviously meeting in the social context of the BAAS brought together those of different political and religious views - as members of the BAAS were well aware. But how far the natural theology as such served to 'paper over' religious differences is far more difficult to ascertain. It will be useful, therefore, to make some comment but restrict this to Sedgwick himself.

It has, of course, to be remembered that (then as now) the most important theological differences were not always denominational. To High Churchmen denominational differences were supreme, but to Evangelicals or to those of a more 'liberal' theological position (for differing reasons) they were not. Sedgwick's religious views were closer to the Church of Scotland Chalmers, the Baptist Robert Hall, or to some of the Quakers, than to the Tractarians. But his affinity for Hall and for the Quakers predated his involvement in science (and in the case of Hall it was shared by the moderate Evangelical Christian Observer - without reference to science). His emphatic rejection of Unitarianism<sup>20</sup> was on the grounds of the Unitarian mutilation of Scripture - which seemed to Sedgwick to undermine the whole of revealed religion. In the instance of Evangelicals like Chalmers and Hall (and many Quakers), Sedgwick shared with them a Christian belief which went far beyond the bounds of natural theology. It may well be that in sharing an interest in science, and seeing science as discovering more of the

world created by the Christian God, Sedgwick and Chalmers felt that much closer together. But this is not what is usually meant by a 'mediating influence'. On the other hand, it may just be possible that Sedgwick might feel himself closer to Unitarians (whose theology he abhorred) if they shared a view of science as discovering a creator God's world - even if that 'God' was not the Christian one in the sense that Sedgwick understood it. The corollary would presumably be that Sedgwick would have felt more affinity with a Unitarian natural theologian than with an atheist scientist. I have not found any way to test this hypothesis one way or the other, though it might well be true.<sup>21</sup>

The only question which one would wish to ask is what exactly was the nature of the 'mediation' intended. Was there any reason why personal animosity should have been felt? Would natural theology have helped on purely social interaction? On the other hand, since natural theology concerns not the content of science but the context in which science is seen, would it have helped in the area of purely scientific exchange? In neither context would it, strictly speaking, be relevant. The only point might perhaps be that it could aid the 'gelling' of personalities in friendship to realise that both saw science as in something like the same context of exploring a Creator's world. It would be better to call this a 'cementing role' rather than (as J.H. Brooke) a 'mediating role', a distinction which is verbal but none the less significant. On the other hand it must be recognised that the specific introduction of natural theological ideas (and even worse of the eulogising of a unitarian like Priestley, and comment on his theological views) could easily have led into discussion of those very contentious areas between unitarians and the orthodox which could breed

dissention.

Surely Sedgwick emphasized natural theology in his Presidential speeches for no other reason than that he genuinely did believe that this was the ultimate glory of science; in a Baconian sense science was the discovery of God's world and told us more of the Creator. But it was a route not an end in itself. To the Christian it was a way to understand more of the God he experienced in Christian grace, to the non-Christian a route to contemplation of the higher truths of revealed religion. It was not seen as a protection against critics, but the *raison d'être* of Sedgwick's own life work in geology. It set in context the scientific endeavours of a man who was primarily a Christian, but one who happened also to be a scientist and saw his scientific work as ultimately having meaning only in the framework of his theological beliefs.

#### 6.4 Notes

1. Orange in McCleod and Collins, Parliament of Science, p. 58.
2. 'The Idols of the Theatre: The British Association and its Early Critics', Annals of Science, 1975, 32, 277-294.
3. Ibid., p. 289.
4. Frank M. Turner, 'The Victorian Conflict between Science and Religion: A Professional Dimension', Isis, 1978, 69, 356-376.
5. Galton, English Men of Science: Their Nature and Nurture (published 1872), see also V.L. Hiltz, 'A Guide to Francis Galton's English Men of Science', Philadelphia Transactions of the American Philosophical Society, 1975, 65, p. 5.
6. J.W. Draper, History of the Conflict Between Religion and Science (1874); A.D. White, The Warfare of Science (1876) (Later editions were retitled A History of the Warfare of Science with Theology in Christendom).
7. McCleod has also written various articles on this, e.g. 'The X-Club: A Social Network of Science in Late-Victorian England', Notes and Records of the Royal Society, 1970, 24, 305-322.  
See also comment in Thackray and Morrell, Gentlemen of Science, and McCleod and Collins, (ref. 1).
8. Criticism has already been made of Gillespie's Charles Darwin and the Problem of Creation for a similarly 'positivistic' approach.
9. It would, of course, be as naive to suggest that a religious orientation is always a help to science as to suggest that it is always a hindrance to it. Darwin later claimed to be a strong theist when he wrote the Origin of Species (Autobiography 1958



Edition p. 93), and it was apparently not a hindrance. His subsequent weakening of theistic belief may have been associated (though not in a simple 'cause-effect' relationship) with an increasingly 'materialistic' view of the origins of man and of human morality (see comment in S. Herbert, 'The Place of Man in the Development of Darwin's Theory of Transmutation', Journal of the History of Biology, 1974, 7, 217-258; also articles by the late Dov Ospovat and his interesting and useful book, The Development of Darwin's Theory (1981) e.g. ch. 3). But, on the other hand, his weakening belief in a Designer also made Darwin doubt the inductive power of his own mind (Autobiography, p. 93 - though Darwin, possibly without consistency, seems only to have directly applied it here to religious induction). Wallace, however, remained involved in Spiritualism at the end of his long life - to the embarrassment of some of his contemporaries. A supreme irony is that whilst Galton was speaking of the pursuit of science being 'uncongenial to the priestly character' (see note 10 below), he was just missing the breakthrough in genetics being made by an Austrian monk whose work would lie unnoticed for a couple of decades.

10. Turner (ref. 4), quotes Huxley to this effect on p. 370, but it is a metaphor appearing elsewhere in Huxley's writings. Galton stated that 'The pursuit of science is uncongenial to the priestly character'. (Galton, ref. 5, p. 24). Doubtless this is true if part of the definition of a 'priestly character' includes unquestioning acceptance of dogma. But, taken more generally, Galton's viewpoint would certainly be challenged from different angles by modern writers such as R. Hooykaas, Religion and the Rise of Modern Science, R.E.D. Clarke, Science and Christian Belief, and S.L. Jaki, Origin of Science and the Science of its Origin.

11. Turner (ref. 4), p. 358.
12. Any opposition to its introduction in education was based on its distraction from more essential subjects, not any idea that it might be harmful.
13. Philip Kitcher's recent book Abusing Science points out the sterility of Creationism (ch.5), a point which was also obvious to those following the accounts of Creationist evidence presented in the recent legal conflict over Arkansas law 590. In Kuhnian terms, it gives rise to no 'normal science'. Kitcher's book is of value as an exposé (if one can accept its sometimes 'Batman' style, and obvious lack of empathy with the general [evangelical] theology of which Creationism is just one version). But its understanding of the historical dimension rests with Gillispie (p. 132: 'For a lucid account of early nineteenth century Flood Geology, see Gillispie'). Differences between modern Creationism and the views of Sedgwick and his contemporaries are sometimes noted (e.g. p. 125), but elsewhere (e.g. p. 1 and p. 4) obscured.
14. Huxley later claimed that Wilberforce had 'crammed' his subject up, and 'knew nothing at first hand' (The Life and Letters of T. H. Huxley, 2, p. 183). The Quarterly Review, however, in 1860, carried Wilberforce's written up critique, and of it Darwin himself wrote in July 1860: 'It is uncommonly clever: it picks out with skill all the most conjectural parts and brings forward well all the difficulties.' (The Life and Letters of Charles Darwin, 2, p. 324).
15. This has been argued already at length for Sedgwick, and is true for others collected in Hull's Darwin and His Critics.
16. It is also interesting to note evidence presented first by Ramm's

The Christian View of Science and Scripture (Ch. 7) and in much more detail J.R. Moore's The Post Darwinian Controversies, that evolution found early acceptance with Evangelicalism and orthodoxy, until a later reversion under Fundamentalism and Creationism.

17. I would include in this Nolan, who for all his recognised scholarship seems a singular figure. Wilberforce commanded much respect and was a power in High Church circles.
18. This has been shown in the previous chapters. For a contrasting study on journals and the Darwinian controversy, see Ellegard, Darwin and the General Reader.
19. Turner seems to be much influenced by the X-club version of the conflict thesis, although professing some doubts of it. In a later Essay Review (Isis, 1982, 73, 563-566) Turner wholeheartedly accepts the Thackray-Morrell thesis of a liberal Anglican domination of science in the period - objecting only to the use of the actual term 'Broad Church' as anachronistic.
20. Clark & Hughes, 2, p. 449.
21. Since, however, we are reduced to speculation, I may be forgiven for suggesting that in my own personal experience religious people are more antagonistic to those whose views appear on the surface to be similar to their own, but are perceived by them to differ on important fundamentals. Heretics are hated more than atheists.

## (7) SEDGWICK, SCIENTIFIC METHODOLOGY AND THEOLOGY

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### 7.1 Methodological Background to Sedgwick's Work

#### 7.1.1 Introduction and Explanation of Approach

Section (7) attempts to explore the way in which Sedgwick's own perceptions of scientific methodology, together with his religious and metaphysical assumptions, interacted with his actual scientific work.

One of the features of his perception of methodology which we will consider in particular is the absence from it of any real concept of the 'theory laden' nature of all scientific observation. From a viewpoint of modern philosophy of science this may be seen as a 'deficiency' in terms of the kinds of explanations which his perception enabled him to make of actual scientific progress (and associated changes in paradigm) in his own field. My own interest and training in the philosophy of science will obviously influence the way in which such issues are presented, though they are surely of a general interest to historians of science.

The first chapter of the section begins with consideration of some early historical background to methodology, for Sedgwick saw himself as very much in the tradition of Bacon and Newton, writing ecstatically to Whewell in 1845: 'It delights us all to think that there is a moral fitness in having the sculptured figures of Bacon and Newton in our house of prayer.'<sup>1</sup> One of my contentions will be that to Sedgwick this was more than lip service, and that certain aspects of, and difficulties in, his own perceptions of scientific methodology have their roots in those of his chosen Mentors. This approach, of course, does rest on a particular view of the methodologies of science implied by the works of Bacon and Newton as they came down to the nineteenth century. A detailed development and defence of these

particular views of Bacon and Newton has been considered out of place here, but they do need briefly stating.

### 7.1.2. Francis Bacon

Bacon was in fact well aware that the senses can be deceptive<sup>2</sup>, and experiment and reason feature highly in his system. The inductive road is, however, long. In over hasty generalisation the mind: 'flies from the senses and particulars to the most general axioms, and from these principles, the truth of which it takes as settled and immovable, proceeds to judgement and to the discovery of the middle axioms...'<sup>3</sup>

The right way 'derives axioms from the senses and particulars, rising by an unbroken ascent, so it arrives at the most general axioms last of all.'<sup>4</sup> Bacon's objection to the Greeks was not that they did not use their senses, but that they were too quick to jump to very general axioms without the ascent through a series of ever more general empirical laws. As we shall see, Sedgwick agreed with him.

There has been controversy over Bacon's attitude to the 'hypothetico-deductive method'. On this I can not agree with Popper who saw Bacon as attacking the method<sup>5</sup>, for what Bacon actually attacks is the construction of an axiom then 'rescued and preserved by some frivolous distinction...'<sup>6</sup> - much as Popper himself would have objected. Neither, however, can I accept Urbach's recent suggestion that Bacon was actually a kind of precursor to Popper<sup>7</sup>, for having rejected a wrong and dogmatic use of conjecture Bacon nowhere goes on to explain a 'right' use. He rather assumes an inductive build up. There is no emphasis on refutable conjectures, unless it is 'read into' his words. Similarly, whilst Ducasse from a modern standpoint argues that Bacon's words are 'tantamount to a declaration' that working hypotheses are necessary, Bacon does not anywhere in fact say so.<sup>8</sup> With hindsight we may find in Bacon a lot of hints and materials for constructing a modern theory of scientific

method - but Bacon himself did not construct one or perceive it in those terms.<sup>9</sup>

From a modern perspective Bacon remains lacking on two counts. First, he (and after him the line of empiricism through Locke and Hume to Mill) failed to take account of the inherently theory-laden nature of observation. Secondly, they make it sound as though actual discovery is mainly a process of ridding the mind of its preconceptions before making observation; in actual fact the process of discovery involves not merely accurately 'reflecting' reality, but actively creating concepts through which to view it. Allied to this and arising out of it is a failure to give any general description of the role of imagination, speculative hypotheses, and corroborated hypotheses, in the methodology of science.

Sedgwick's writings show that he had read Bacon, and regarded his own science as an application of Baconian principles. But the Bacon which he read was not perceived through a modern framework of methodology, and Sedgwick did not 'read into' Bacon any of the principles which I have described as self evident today but only hinted at by Bacon. Thus, whilst today we might 'see' the paradigm changes in geology during the nineteenth century in (say) Popperian or Kuhnian terms, Sedgwick tried to interpret them using what he actually found in Bacon (and, as we shall see, also in Newton).

Finally, some comment might be made on Bacon's views on science and religion. Study of nature leads us to magnify the works of God.<sup>10</sup> God has given us two books: 'the Scriptures revealing the will of God, and then the creatures expressing his power.'<sup>11</sup> This idea, as we shall see, was influential on Sedgwick. Likewise was Bacon's insistence that we could not 'find the truth of natural philosophy

in the Scriptures....'<sup>12</sup> The Bible is not designed to teach what today we call science. Nevertheless, ultimately nature and revelation must concur - and we need not fear the most thorough study of nature.<sup>13</sup> He did, however, warn against a too hasty synthesis, for such systems tended to become conservative and view criticisms of the science as criticisms of the religion implicit in them.<sup>14</sup>

### 7.1.3 Newton

What we are really interested in here is not so much the complex picture of Newton emerging after rediscovery of many of his papers<sup>15</sup> but the view of him which came down to the nineteenth century. Though some other material (like his correspondence with Cotes and Bentley) was available, most scientists then judged him on his Principia and his Opticks.<sup>16</sup>

On his metaphysics it is important to qualify statements like those of Koyre about the 'unholy alliance of Newton and Locke' which 'Produced an atomic psychology, which explained (or explained away) mind as a mosaic of "sensations" and "ideas" linked together by laws of association (attraction)...'<sup>17</sup> The reductionism of Lockian empiricism as interpreted by Hume achieved a monistic ontology at the expense of a coherent or convincing treatment of consciousness. Men like Sedgwick and Whewell utterly rejected this, turning effectively to a kind of dualism not dissimilar to that of the Cambridge Platonists with innate conscience faculties. Yet, having rejected Lockian empiricism, Sedgwick was able to idolise Newton. This makes sense only if we recognise firstly that Locke himself was concerned with epistemology rather than ontology,<sup>18</sup> and secondly that Newton's was fundamentally a monistic idealistic version of Locke, with all physical agencies explained as the immediate will of God.<sup>19</sup> In his actual science, of course, Newton, like most idealists, treated the physical

world as though it were 'real'.<sup>20</sup>

On scientific methodology Newton's famous 'Rules of Reasoning' added to principles of economy of concept and universality the words:

'In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate or liable to exceptions.'

This is in a sense hypothetico-deductive, but the way hypotheses are presumed to come is by 'making experiments and observations, and in drawing general conclusions from them by induction...'<sup>21</sup> Induction, not conjecture, is their source - and they are tentative only in view of possible 'exception' rather than by paradigm change. Various views have been expressed on relationships between Newton and Baconianism, and Newton and modern falsificationism.<sup>22</sup> Feyerabend, I believe rightly, sees him as nearer to a mild brand of Nagelian positivism than to falsificationism.<sup>23</sup> This lays him open by implication to the same kind of criticism - he makes no allowance for a Kuhnian type 'gestalt-switch' paradigm change, and takes no cognisance of the 'theory-laden' nature of observation. This lacuna is, I would argue, reflected in the ideas of his follower Adam Sedgwick.

Newton's attitudes to hypotheses have been the subject of much discussion, and Koyre especially has followed his changing views and use of that term.<sup>24</sup> Hypothesis were always to be distinguished from laws directly induced from phenomena, and only the latter were certain.<sup>25</sup>

What we are left with, then, is that Newton's view is this: Experimental philosophy proceeds by deducing causes from effects, an induction from observation to produce 'theories'. These are extended or restricted by further observations, and in practice may be harmonised with apparently refuting evidence by means of 'hypotheses'



- these hypotheses being used simply to explain apparent discrepancy. Empirical theories, once found, are never replaced, only extended or restricted. Hypotheses concern concepts which cannot be deduced from phenomena; they are used to explain or to suggest experiments, but never achieve the certainty of theories.

This is not a Popperian version of a Hypothetico-deductive model, nor can it handle Kuhnian 'revolution' - unless one reads this as a change of hypothesis whilst preserving the same theories.

#### 7.1.4 Methodological Heritage

Bacon and Newton were the key figures in conceptions of scientific methodology in the era of early Sedgwick. The eighteenth century had seen the rise of Hume's reductionist empiricism, and Kant's transcendental idealism. But Hume offered little to the empirical scientist, and whilst Kant's philosophy formed a launching point for the Naturphilosophen (with some parallel's in Coleridge's science), Sedgwick abhorred any such approach to science.<sup>26</sup> Sedgwick refers seldom to Kant and (unlike Whewell who was rare in this respect) his first hand knowledge of Kant is questionable.<sup>27</sup> His perceptions of scientific methodology, particularly in his early period, were based on Bacon and Newton whom he took for his mentors. Though, moreover, he was a man of his times, the marks of this early approach always remained with him, and the strengths and weaknesses of their methodologies as ways of perceiving what was actually happening in science are apparent in his own interpretations of scientific developments.

## 7.1 Notes

1. In the Trinity Whewell collection, Add. Ms. A 68/121 dated November 7th 1845.
2. See e.g. The Great Instauration, see also R.M. Blake, et al, Theories of Scientific Method, ch. 4.
3. Novum Organum: Aphorisms xix.
4. Ibid.
5. Conjectures and Refutations, p. 14; the most important Baconian references are in Novum Organum 1, and the phrase 'anticipations of nature' referred to by Popper appears in xxvi, xix-xxii, lxiv, cvi, etc.
6. Novum Organum, 1, xv; see also Mary Horton, 'In Defence of Francis Bacon', Studies in the History and Philosophy of Science, 1973, 4, 241-278.
7. P. Urbach, 'Francis Bacon as Precursor to Popper', Brit. Jour Phil. Sci., 1982, 33, 113-132.
8. In Blake (ref. 2), p. 72.
9. Perhaps one of the clearest passages is: '... by successive steps not interrupted or broken, we rise from particulars to lesser axioms; and then to middle axioms, one above the other; and last of all to the most general. For the lowest axioms differ but slightly from bare experience, whilst the highest and most general ... are the notional and without solidity...' Novum Organum, 1, civ. This seems to be the kind of thing meant by Sedgwick in his emphasis on the 'inductive track'.
10. See e.g. Valerius Terminus, Cap 1.
11. Ibid.
12. Of the Advancement of Learning, Bk. 11., see also Novum Organum, 1, xv.

- and De Augmentis Scientiarum, Bk. 9, ch. 1.
13. See Filum Labrinthi, 7; Novum Organum, 1. lxxxix.
  14. Novum Organum, 1, lxxxix. In this connection Bacon objected not only to deriving science from Scripture, but also to using final causes as part of the mechanisms of physics (see De Augmentis Scientiarum, 3, iv). Ultimately the physical creation pointed to a Creator; but natural philosophy should study secondary causal patterns without introducing final causes at this level.
  15. See e.g. A.R. and M.B. Hall, The Unpublished Scientific Papers of Isaac Newton, 1978 Edition, P. and R. Wallis, Newton and Newtoniana 1672-1975 (published 1977), etc.
  16. The book auction catalogue for Sedgwick's book collection after his death lists the second edition of Principia (1713), and Edleston's edition (1850) of the Cotes correspondance. Sedgwick's Discourse (1st Edition p. 85) refers to Newton's Opticks.
  17. Koyre, Newtonian Studies, p. 23.
  18. See e.g. R.L. Armstrong, 'The Cambridge Platonists and Locke on Innate Ideas', Journal of the History of Ideas, 1969, 30, p. 193.
  19. See e.g. Westfall, Never At Rest, p. 303, Newton's De Gravitatione et Aequipondio Fluidorum (in Hall and Hall, ref. 15), etc.
  20. Hall and Hall (ref. 15) make this point, p. 81.
  21. Queries 31.
  22. See e.g. Koyre (ref. 17) p. 40 and Brewster, Memoirs of the Life, Writings and Discoveries of Sir Isaac Newton, 2, p. 403, for opposite views; also Blake (ref. 3), p. 140, Westfall (ref. 19), p. 167 n69, Kuhn in Cohen, Franklin and Newton, pp. 37-38, etc.
- A further point made by several writers is that Newton quantified.

23. In R.E. Butts and J.W. Davis, The Methodological Heritage of Newton, p. 160n (see also Säbra, Theories of Light from Descartes to Newton, ch. 2). Nagel strongly distinguished 'theories' (parallel to Newton's 'hypotheses') from 'laws' (parallel to Newton's 'theories'). To Nagel 'Laws' concern directly observable entities, theories are constructed to explain laws. In practice, however, such a system has difficulty accomodating the theory-laden nature of observation, and Nagel is reduced to a 'sleight of hand' substitution of 'established procedures' instead of 'observably identifiable traits' on successive pages (pp. 83-84, The Structure of Science). This is interesting as we shall see Sedgwick (below on p. 431) having a similar kind of problem.
24. Koyre (ref. 17), pp. 30-31 etc, see also Cohen (ref. 22), who catalogues uses in an appendix, though Hanson in ch 1 of Butts & David (ref. 23) is critical of this.
25. Newton wrote to Fontenelle: 'I cultivate the experimental philosophy, as that which is worthy to be called philosophy, and I treat hypothetical philosophy, not as knowledge, but by means of queries.' (cited in Westfall (ref. 19), p. 792.)
26. See below p. 476 etc.
27. See below p. 465 etc.

## 7.2 The Social-Scientific Background to Sedgwick's Work

### 7.2.1 The Background

Sedgwick, who previously had exhibited no major interest in geology, was elected Woodwardian Professor in 1818. I have already given in chapter 5.2 some general background of geology; but our interest then was as a background to natural theology whereas our present focus is on geological methodology. For this reason some elements will have here to be expanded.

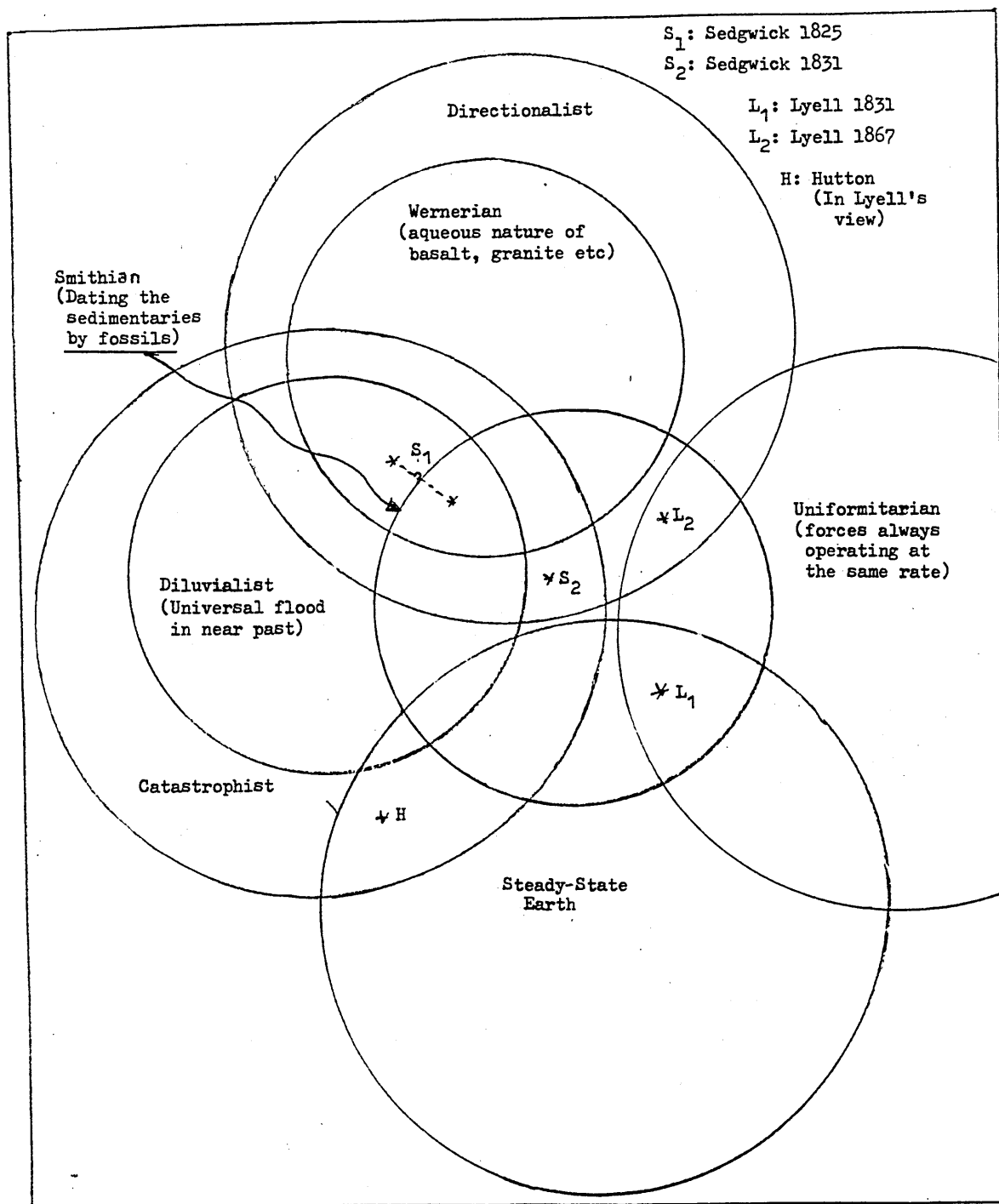
Lyell, in his Principles, cultivated a view of geological history with Hutton as a kind of prophet and himself as a geological messiah. Recent historical study, however, has shown that whilst this propaganda has dominated resumés of geological history even into the 1970's, it is clearly a distortion.<sup>1</sup> Its presentation of Wernerianism as a kind of rash and autocratically maintained speculation (as against the sober induction of Hutton) is particularly misleading<sup>2</sup>, but it also obscures a number of important methodological distinctions (e.g. actualism vs uniformitarianism).

In fact the relationships between the key ideas in this phase of geology are complex. We need to consider them under two aspects: their relationships as ideas in the abstract, and the actual historical contexts and associations in which they arose. To give the discussion a visual focus I have represented these key ideas by means of a Venn diagram (Figure 7.1). To this the following comment may be added.

One term which is not shown and has no separate circle is 'actualism'. Rudwick defined this as 'the methodology of inferring the nature of past events by analogy with processes observable in action at the present'.<sup>3</sup> Now the early schemes had not always been actualist (Woodward, for example, found himself forced into a view

Figure 7.1

Early Nineteenth Century Geological Ideas in a Venn Diagram



that gravity itself was suspended during the flood), but by the early nineteenth century all serious geologists used actualist principles where they could. Sedgwick certainly never ever considered anything else. But this is to be distinguished from uniformitarianism, which is the view that the levels of forces operating have always been approximately the same.

The earlier controversy was between the Wernerians and Huttonians. Two major issues divided them. First there was the directionalism of Werner vs the steady-state ideas of Hutton. Secondly there was the Wernerian view that virtually all strata were aqueous in origin vs Hutton's view that many were igneous. The two were connected. Werner observed that certain types of rock (e.g. granite and gneiss and basalt) did not contain fossils. Both lack of fossils and peculiarities of lithology could therefore be explained if these were 'primitive' rocks, precipitated out of a deep sea before life appeared. Next there followed a period of 'transition' rocks, some precipitated and some laid down mechanically, a more stormy period where fossils appear showing the existence of life. Then in the next period, the 'floetz' period, life teemed in a stormy, low standing ocean, and fossils abound. Finally, the most recent period is one in which volcanic action and alluvial deposits have made local modifications to older rocks. This view sees most rocks as aqueous in origin, and it also has 'directionality' or 'progression'. We must, in fact, be careful in using such terms, for it lacks the strong sense as used by later Darwinians. But Wernerians saw a time when there was no life on earth, and later came to recognise that higher animals such as vertebrates appeared later in the record. Hutton had no clear idea of associating fossil types with stages of earth history, and in his much quoted words said: 'In the economy of

the world I can find no traces of a beginning, no prospect of an end.'<sup>4</sup> Lyell saw Hutton as a kind of catastrophist, for 'He imagined that the continents were first gradually destroyed, and when their ruins had furnished materials for new continents, they were upheaved by violent and paroxysmal convulsions.' Nevertheless it was a steady-state earth; these catastrophic cycles had no directionality; they remained cycles. Werner had explained the lack of fossils in rocks such as granite, gneiss and basalt in terms of his directional system, seeing them as 'primitive'. Hutton saw them as of igneous origin, putting igneous action more on a par with aqueous in importance.

Neither Wernerianism nor Huttonianism were self evident, and neither proved wholly 'correct'. Trap rocks (now recognised as igneous intrusions into older beds) could be explained by Wernerians as being precipitated from above. Werner had not assumed a uniform progression of universal conditions, and what is now 'seen' as obvious disproof (the metamorphosis of other beds in contact with igneous intrusions) was by no means observationally clear and was often not an obvious result of heat.<sup>5</sup>

Wernerianism had two important implications. The first was that geology involved not only a study of operations going on over the earth's surface, but that it offered a genuine study of the history of that surface's development. Secondly, lithology and mineral type were important in that they provided a key to dating rocks.

The term 'catastrophist' has already been used. It was first invented by Whewell, who derived it thus: 'That great changes of a kind and intensity quite different from the common course of events, and which may therefore properly be called catastrophes, have taken place upon the earth's surface, was an opinion which appeared to



be forced upon men by obvious facts.'<sup>6</sup> Whewell also uses the term 'uniformity', in particular of the views of Prévost and Lyell. But his treatment shows a consciousness that the distinction is not clearcut. At the extremes (in, say, early Buckland and Lyell) the distinction is clear enough, but there may be many intermediate positions. Diluvialism is a particular kind of Catastrophism - or perhaps one should rather say a genus as there are different species of Diluvialism. The work of Cuvier and Brongniart on the Paris basin led them to picture alternate states of marine and freshwater conditions, with a change of fossil species between each horizon. Cuvier pictured these as sudden changes, with a continent-wide catastrophe introducing new conditions.<sup>7</sup> Jameson, who translated Cuvier's work, identified the last of these catastrophes with the Noachic flood. In this form it was adapted by Buckland, who from 1819 pictured a series of world wide floods, the last of which was to be identified with the flood of Noah.<sup>8</sup> We shall later see the inductive base for this; but it is important to recognise that it was only one form of catastrophism, and indeed only one form of diluvialism. Sedgwick had abandoned it by 1831, and it was dead after Buckland himself quietly dropped it in 1836. But Sedgwick continued to believe in diluvial action on a local level.<sup>9</sup> Later, Agassiz' glacial theory and the concept of ice ages could be seen as a form of catastrophism. One of the practical corollaries of the catastrophist-uniformitarian dispute was attitude to epochs of the earth's history. Catastrophes would imply widespread discontinuities in the stratigraphical records - especially in terms of a directional view of fossils.

The role of fossils is, indeed, the last issue represented on Figure 7.1. The fact that particular fossil types are associated

with particular strata was noticed in the eighteenth century by some observers.<sup>10</sup> In general, however, neither Huttonians nor Wernerians had utilised this fact very much. It seemed inherently improbable that the correlation would be universal. The development of the assumption of such a correlation as a key tool in identifying strata seems to have come from two sources. An English land surveyor, William Smith, developed from about 1790 the technique of making a major use of fossils to identify strata.<sup>11</sup> The actual degree to which he used fossils alone was a major element of the Sedgwick-Murchison controversy, and modern studies have doubted it.<sup>12</sup> It is also to be doubted whether he in any sense saw the method as directionist - most of his fossils were invertebrates.<sup>13</sup> But what he did do was to undertake geological mapping in a major way. His actual techniques and ideas were disseminated only by word of mouth before 1815, but in that year he published his beautiful map of the strata of England and Wales.

There seems no reason to doubt either that Smith made his discovery of the method independently, or the magnitude of his achievement in the production of his map. The often quoted eulogy Sedgwick gave him in his Presidential Address of 1831 was well deserved. But the impression sometimes given of this 'father of English stratigraphy' is that it was his published work which brought a revolution in stratigraphical thinking. This is misleading. Cuvier had recognised as early as 1801 that the older the beds the greater differences between fossil and present species<sup>14</sup>, and both Cuvier and Brongniart had done work on the identification of strata from fossils from around 1808.<sup>15</sup> In Cuvier and Brongniart's work this facet was associated with both directionalism and a form of catastrophism. In any event, there was no instantaneous acceptance by geologists

of an invariable correlation of fossils and strata.

A few figures have been tentatively plotted on the diagram. Sedgwick and Lyell both, though at different times, moved from one intersection to another - though their paths did not cross. Hutton has also been tentatively placed. Murchison seems to have moved over (effectively) to join Sedgwick from an initially less Catastrophist position. As we shall see, Murchison was (or became) more extreme in what (after Farey) I have called 'Smithian' doctrines. The Venn diagram has to dichotomise - and sometimes these issues were a matter of degree. Hutton too is seen by Lyell (effectively) as a kind of catastrophist - but others have seen him as uniformitarian. Most visual devices, however, have some limitations.

Whilst 'shades of commitment' within each zone might be more accurate it would be impracticable since over-complex. But at least the existing diagram is right in leaving a geologist free not to have made up his mind on some of the issues. The Geological Society of London was, as has often been said, founded in 1807 to encourage practical work in stratigraphy rather than theory or speculation about causes of strata.<sup>16</sup> Its early membership included all parties: Wernarians (e.g. Jameson), Huttonians (e.g. Playfair), Smithians (e.g. Townsend).<sup>17</sup> Its social nature did discourage the membership of engineers and surveyors (e.g. Smith, Farey, and Bakewell), though this did not stop a fruitful interchange of ideas. Rachel Laudan has actually argued that in its early years it contributed to a stagnation and deterioration of geology (though this is strange since she also says that in 1807 English geology was already 'at a particularly low ebb')<sup>18</sup> She sees the 'Baconian' strictly inductive method and opposition to theory as one of the less useful methodologies available. According to Laudan its early members dismissed the work

of such as Werner, Kirwan, Deluc and Hutton as 'idle speculation', and thought that, since anyone could gather facts, ignorance of geological principles was no bar to membership.<sup>19</sup> Laudan's analysis, however, seems to rely very heavily on the viewpoint of the Society's first president Greenough. But Greenough was particularly extreme. He was not only suspicious of the inevitable correlation of fossils and strata (as were many geologists)<sup>20</sup>, but tried (as Laudan well shows) to avoid any theoretical assumptions whatever, even when he was forced to use Smith's map to compile his own. Rudwick has recognised his singularity (especially as the science advanced) by virtually giving him a style to himself<sup>21</sup>, and his singularity made him appeal especially to others with a different interest in debunking existing geological theory, such as Nolan. But Laudan gives little evidence of so extreme a viewpoint in the Society for others than Greenough.<sup>22</sup> So, MacCullough, for example (an early President and influential on Sedgwick) suggests causal mechanisms in his 1817 contribution on the parallel roads of Glen Roy.<sup>23</sup> The Society's widely drawn membership apparently remained committed to their own views - Wernerian, Huttonian or Smithian - and Laudan's suggestion that they dismissed the work of their predecessors as 'idle speculation' is in general simply untrue. New members such as Sedgwick (1818), Lyell (1819) and Murchison (1824) are said to have seen themselves in the 1830's to be in a struggle with the 'old guard'.<sup>24</sup> Laudan, then, sees three distinct phases: (i) Theory and Controversy (pre 1807) (ii) Stagnation due to the Society (1807-1820) in reaction against 'idle speculation' of predecessors (iii) Reaction against 'old guard' (1820-1830's). But this 'catastrophist' view of geological history is too heavily based on Greenough. The Geological Society brought a change of emphasis - which was surely beneficial in enabling geologists of all theoretical persuasions to work together - but there is an essential

continuity throughout the whole period. Sedgwick began as a Wernerian, always retained some elements of the Wernerian approach, and also saw himself as acting on Baconian principles much as the Society had always claimed.

A more useful article showing the position of the Society within the wider framework of a perceived social and academic structure of geological study has recently been written by Rudwick.<sup>25</sup> There is no need here to reproduce his full argument and diagram, but he pictures the social topography of geology in the 1830's as a 'series of graduated zones of "ascribed competence, that shade insensibly into one another.' A central elite are the big guns (e.g. de La Beche, Lyell, Murchison, Sedgwick and de Beaumont) whose opinions are treated with great seriousness even in the midst of any controversy. An outer circle of 'accredited' geologists included men (e.g. Whewell or Herschel) whose expertise impinged in an auxiliary way on geology, or men with local knowledge (e.g. Phillips<sup>26</sup>). Outside this, Rudwick portrays a zone of 'amateurs', people with purely factual knowledge at the local level but whose theoretical views commanded no particular respect. The word 'amateur' is unfortunate, for to us today it implies a distinction between someone who does ~~or~~ does not earn his living in the activity; in the early 1830's there were no 'professionals' in this sense.<sup>27</sup> Even after the commencement in 1835 of paid workers for the Government Survey the salaries were not sufficient for total support.<sup>28</sup> Perhaps 'geologically untrained' might be a better phrase, though this in turn conjures up pictures in our modern minds of geological degree courses which would be equally misleading.<sup>29</sup> Whatever caveats, however, one puts on the terms, Rudwick shows how the Geological Society included people in all the three levels.<sup>30</sup>

A more speculative visual schema is given in Rudwick's

article on cognitive styles in geology.<sup>31</sup> Again it is impossible to reproduce the whole argument here, but the two styles most interesting to us are perhaps the 'Abstract' and the 'Concrete'. The Abstract (e.g. Lyell, Prevost, Scrope, Phillips and Darwin) has a weakly classified earth-history with emphasis on the time dimension and causal mechanisms and a belief in its ultimate simplicity. The Concrete style (e.g. Brongniart, Buckland, Conybeare, Sedgwick, and Murchison) focus on the structure rather than time and causal mechanisms, suspecting generalisation and emphasising empirical fact-gathering to ascertain the complexities of the system studied. These are stimulating ideas, and relate intimately, to generalisation. But there must be many exceptions. On 'time', for example, Sedgwick's Third Edition Syllabus (1837) has a section:

'Time during which the above-mentioned physical agents have been at work - natural chronometers - difficulties in their application - examples.'<sup>32</sup>

It was perhaps not disinterest but scepticism which kept Sedgwick from thinking in terms of 'Newtonian time'. To take Rudwick's analogy, it may not be that (like the Nuer) Sedgwick would have had no conception of Newtonian time, but he might instead be compared to Robinson Crusoe marooned in Greenland without a watch. It is not the conception but the means of measurement he lacks.<sup>33</sup> Concerning generalisation it is strange that de Beaumont appears in the Concrete group. Perhaps it is not so much generalisation, but simplifying assumption which splits the two groups. In a sense this associates with the search for a chronometer - a man can construct one only if he makes simplifying assumptions. Rudwick compares the belief in the ultimate simplicity of the world to Popperian bold conjecture,

but this may be misleading. The point is that with Popper's bold conjecture he meant conjecture that makes predictions specific enough to risk refutation. But Rudwick has told us that anomalies are assimilated - accepted in confidence that ultimately they will be resolved. A true Popperian (if there ever has been such a thing in science) would consider the theory refuted. Besides, it is not clear how (say) Lyell's uniformitarianism or Darwin's evolutionary theory 'stuck their necks out'. Both were rescued simply by ad hoc hypotheses when threatened. The question of speculation is, however, a key one, and will need reconsideration in the light of more specific Sedgwick material.

On causal explanation Sedgwick shows some interest - more especially in books of a more general nature e.g. his part in Hudson's Guide to the Lakes.<sup>34</sup> Again, chapter III (pages 5-9) of Sedgwick's Third Edition Syllabus (also a general introduction) is entitled: 'On the great agents by which the Earth's surface is modified; and on the effects which have been produced by them during known periods'. Many known modern examples are listed, and Sedgwick evidently spoke of past causes (including the parallel roads of Glen Roy). If he differs from the 'Abstract Style' here it can only be in degree.

Finally, some question might be raised over Rudwick's assertion that the Abstract Style is typically that of the 'Big Men' of geology, anxious to secure their own reputations. The difficulties of assessing its validity are enhanced by the fact that other more general trends interfere. The earlier phase of stratigraphy (say up until 1850) in this country was an era of colourful individualists, a heroic age. The second half of the century was an era when no big systems remained unnamed, and when the accent turned (in stratigraphy

at least) to more detailed work. The days of the possibility of a Murchison were over. So there is a time dimension in the possible openings to be a Big Man. Secondly, there is a certain amount of implied tautology in the association of Big Men with a more speculative turn of mind. Apart from the occasional Murchison who (as even the loyal Geikie admits) was not the greatest theoretician but was energetic, or possibly the occasional William Smith, the recognition of a Big Man is likely to go to someone who revolutionises theory. To become a Big Man based on actual empirical work one has to be 'in' very early - and the work itself is usually soon superceded by teams of people. But of those listed, Buckland could be seen as a Big Man, Sedgwick might have been if he had ever had the time to write his Big Book on the Paleozoics before it was too late, and Murchison (as even Rudwick has to admit) is the archetype. On the other side, Darwin and Lyell certainly were, but Phillips surely was not? The samples, even if we include the foreign geologists, are really too small and too inconclusive for us to infer any significant difference. There are other possible theories of Big Men but it may finally come down to personality, at least if our focus is on motivation rather than success in obtaining recognition. Whether greater personal ambition is correlated with a more theoretical and speculative attitude of mind is difficult to say.

These kinds of cognitive and perceptual frameworks do, however, have a useful stimulating function as one considers the life and work of an individual like Sedgwick.

### 7.2.2 Contexts and Periods for Sedgwick on Methodology

The major concern of the present section is the methodology of Sedgwick. We have now see the background both of the ideas which he imbued (particularly of Newton and Bacon), and of the science



in which he was involved. Sedwick comments on his approach to methodology in a number of particular contexts:

- (i) In general apologetic for and explanation of geology  
(e.g. in his lectures and in the Discourse)
- (ii) In connection with his own geological development  
(e.g. abandoning Diluvialism)
- (iii) In geological **controversy**  
(e.g. the controversy with Murchison)
- (iv) In reaction to Scriptural Geologists  
(e.g. against Cockburn)
- (v) In reaction to evolutionary theories  
(e.g. Chambers or Darwin)

At the same time we may also distinguish different periods of Sedgwick's life:

- (a) 1818-1830: The early period, during which he was a Buckland Diluvian and (to begin with) a Wernerian. His publications included his articles on Diluvialism (1825), and also his first Syllabus (1821). The period ends with his public recantation of Diluvialism in his Presidential Address of 1831
- (b) c1831-c1852: A middle period, during which the B.A.A.S. was formed, Sedgwick entered polemics both with Scriptural geology and with the Vestiges, and the first edition of his Discourse was issued. The Sedgwick-Murchison controversy was then developing its first phase, and Sedgwick's *specific* methodology for stratigraphical recognition was not settled.
- (c) c1853- ? : A last period of active life. The stratigraphical facts of Wales were now established (with the mistakes McCoy and Sedgwick himself exposed in 1852), and the Sedgwick Murchison controversy was about principles and personalities.

Sedgwick's fifth edition Discourse was published (with its vast Introduction and Appendices). Sedgwick entered controversy over Darwinism.

His general attitude to methodology remained remarkably undeveloped during this time. One might point out also that Herchel's Preliminary Discourse was published in 1831, and the much more sophisticated Philosophy of the Inductive Sciences was dedicated to Sedgwick in 1840 by Whewell. Neither seem to have much affected Sedgwick's philosophy of science.

## 7.2 Notes

1. Rudwick, in 'The Strategy of Lyell's Principles of Geology', Isis, 1970, 61, 4-33, states that: 'Lyell's highly "Whiggish" interpretation of the history of geology naturally entails distortion and oversimplification of the theories of his predecessors.' (p. 10). Michael Bartholemew's work on Lyell has tended to expand on this, and see also Roy Porter, 'Charles Lyell and the Principles of the History of Geology', BJHS, 1976, 9, 92-103.
2. Even the recent (1982) biography of Sedgwick by Colin Speakman (in spite of its credits to Porter and Morrell and some well researched comment on Sedgwick) presents Werner (apparently on Geikie's authority) as a poorly travelled 'Pope' who taught neptunism and whose ideas were abandoned by 'the younger more radical scholars'. For the explosion of the myth see Ospovat in 'The Distortion of Werner in Lyell's Principles of Geology', BJHS, 1976, 6, 190-198 and also in the Dictionary of Scientific Biography.
3. Rudwick: The Meaning of Fossils p. 110; the confusion between actualism and uniformitarianism in the work of Lyell has already been indicated; Rudwick, in a pioneering chapter in Perspectives in the History of Science and Technology (Ed. D.H. Roller, 1971) identified several different meanings to the term 'uniformitarian' all confused in Lyell. Rudwick also utilised the term 'directionalist' in preference to 'progressivist' because of its more neutral implications.
4. Lyell cites this approvingly in Principles, 1, p. 63, and Rudwick has argued that it was central to Lyell's concern. For some recent

comment on Hutton's methods see J.E. O' Rourke, 'A Comparison of James Hutton's Principles of Knowledge and Theory of the Earth' Isis, 1978, 89, 5-20.

5. See Rudwick, 'Hutton and Werner Compared: George Greenough's Geological Tour of Scotland in 1805', BJHS, 1962, 1, 117-135.
6. Whewell, History of the Inductive Sciences, iii, p. 506.
7. Cuvier's work with Brongniart began to be published in 1808, and the more general Recherches sur les ossements fossiles on palaeontology in 1812. The preliminary discourse to the latter appeared in Jameson's English translation in 1813 as Essay on the Theory of the Earth. Sedgwick was not, of course, reliant on this translation, and referred to the French original.
8. This was particularly in Buckland, Vindiciae Geologicae (1820). Buckland is much less circumspect in his language than Sedgwick, speaking of 'the proofs of the Mosaic deluge presented by natural phenomena' (Op Cit p. 36). Buckland does believe the physical evidence stands independently of the Mosaic account, but: 'the grand fact of an universal deluge at no very remote period is proved on grounds so decisive and incontrovertible' that physical grounds alone would be sufficient.' (p. 23).
9. Sedgwick wrote to Murchison in 1837 expressing incredulity at Buckland's apparent continued attachment to diluvialism although it had been dropped in the 1836 Bridgewater. He added: 'I am still a Diluvialist in a certain sense. But every (?) of drift must be referred to its own cause.' (Unpublished letter in the Murchison correspondence in the Geological Society.).
10. This was known to Sedgwick's contemporaries as Whewell (ref. 6, iii, p. 443) cites Fonelle pre 1770.

11. Smith himself dated it from 1790 (see Philosophical Magazine, 1833, 1, p. 38 etc.; see also the Dictionary of Scientific Biography entry and citations there).
12. See particularly Rachel Laudan, 'William Smith: Stratigraphy Without Palaeontology', Centaurus, 1976, 20, 210-226; Laudan argues that Smith first used lithological characteristics.
13. V.J. Eyles (Dictionary of Scientific Biography p. 489) and Olroyd 'Rise of Historical Geology', Hist. Sc., 1979, 17, p. 241) take different views on this. The word 'directionalist' has been used here in preference to 'progressivist' for reasons already indicated. Peter Bowler (Fossils and Progress p. 35) also discusses this, arguing that 'progression' was never an important part of the concept and explaining Sedgwick's apparently inconsistent argument for directionalism against Lyell's steady state, but against it when confronting the Vestiges evolutionary schema.
14. See e.g. The Meaning of Fossils, p. 127.
15. This was referred to in their essay on the mineral geography of the Paris region (1808); c.f. The Meaning of Fossils p. 128-29 where it is suggested that Cuvier and Brongniart may have been influenced by a notice of Smith's work. Rudwick also shows ('A Visual Language for Geology', Hist. Sc., 1976, 14, p. 170 etc.) that 'the visual conventions of Cuvier and Brongniart's sections were quickly adopted by the Geological Society of London' in preference to those of William Smith.
16. H.B. Woodward's classic history, The History of the Geological Society of London (1907) made this point, and in general it has been supported by recent scholarship. (e.g. Rudwick, 'The Foundation of the Geological Society of London; Its

Scheme for Cooperative Research and Struggle for Survival,  
BJHS, 1963, 1, 325-355.)

17. See Rudwick (ref. 16, p. 329); the neptunist Kirwan also joined.
18. Laudan, 'Ideas and Organisations in British Geology; A Case Study in Institutional History', Isis, 1977, 68, 527-538.
19. Laudan, Ibid., p. 531.
20. Greenough, A Critical Examination of the First Principles of Geology in a Series of Essays (1819) - c.f. pp. 287-8.
21. Rudwick, 'Cognitive Styles in Geology', in Essays in the Sociology of Perception, (Ed. M. Douglas 1982).
22. Though Laudan states that, unlike other groups who paid lip service to Baconian principles, in the Geological Society 'the members actually acted on their principles' (p. 530). No actual evidence that this was a general trait is produced.
23. Transactions of the Geological Society, 1817, 4, 314-92.  
 MacCullough was certainly active throughout this period, and since Sedgwick later acknowledged his influence (as we shall see) MacCullough's influence should not be underestimated.
24. Laudan (ref. 18), p. 537.
25. Rudwick, 'Charles Darwin in London: The Integration of Public and Private Science', Isis, 1982, 73, p. 190 etc.
26. Rudwick emphasizes the gradation of the zones. Some might surely see Phillips as approaching the elite (for most, even of the elite, specialised on some age of strata e.g. Lyell on the Tertiary and Sedgwick on the Palaeozoic). Others, like Otley and Lewis (classed by Rudwick as amateur) might border

between accredited and amateur.

27. There may have been some (like Mary Anning) who made a living collecting and selling fossils - but these would be classed as 'amateurs' in Rudwick's schema.
28. Roy Porter makes this point in his article, 'Gentlemen and Geology', (Historical Journal, 1978, 21, 809-836).
29. Colin A. Russell gives some interesting comment on the multi-faceted meaning of 'professional' in his recent Science and Social Change 1700-1900, p. 220. See also e.g. D.M. Knight 'Science and Professionalism in England, 1770-1830', Proc. XIVth Int. Congress Hist. Sci, Tokyo, 1974, 1, 53-67.
30. His portrayal of the Scriptural Geologists as mutually exclusive to the Society is not 100% accurate, for in 1830 Andrew Ure was a member - to the scandal of Sedgwick who was President that year. But Ure is an interesting special case: a man who had edited a dictionary of chemistry, was an early participant in the BAAS and had one of the earliest visions of a fully automated factory.
31. Rudwick (ref. 21).
32. Sedgwick, Syllabus, p. 7.
33. Rudwick actually says: 'The focus of attention is the concrete order of strata and other rocks, rather than the temporal earth history they represent'. Taken in a strong sense this would be at odds with the fact that it was these very geologists who emphasized the appearance of new species as a proof that God continued to act in his created world in periods after the creation. It is surely the question of whether a ratio rather than an

ordinal scale of time measurement could be constructed. Both groups thought in some sense temporally, Lyell himself (in the 10th edition of his Principles in the late 60's) admitted it was hopeless to 'assign a chronological period to any period but the latest' though still prepared to speculate (see the references in Rudwick, and especially 'Lyell's Chronological Model: Published Year Values for Chronological Time', Isis, 1977, 243, pp. 440-3.)

34. Hudson, Guide to The Lakes, p. 214, states:

'Before I attempt any sketch of the older slate rocks of the Cumbrian mountains, let me endeavour to translate into common language that chapter in the strange old chronicles of the earth, of which we have been turning over the leaves from the end to the beginning.

First then, we have the record of an ancient revolution given by the old conglomerates - Secondly, the great scar limestone tells us of a long period of repose. Its coral reefs were formed in a shallow sea (for in such seas only do corals grow) but in course of time it sank down, and a sea many hundred feet deep floated over it, and spread out upon its banks of sand and mud...'

Before saying he will not leave the 'slippery ground of hypothesis' Sedgwick adds several more scenarios. He also says later (p. 243):

'The great formations of geology, however varied in their features or imposing in their combination, derive their chief interest from being the monuments of successive periods of time ...'



### 7.3 Sedgwick's Early Work

#### 7.3.1 Sedgwick and the Ideas of Werner and Smith

We may begin with some of Sedgwick's own later comment on his early work. In an unpublished autobiographical fragment he wrote:

'The Wernerian controversy was then [1821] at its height... I took McCulloch for my guide and saw much under his point of view. I became therefore for the time a firm adherent to the Wernerian doctrine and in 1819 I published a paper declaring my belief that granite veins were aqueous. I went to Cornwall with a somewhat guilty [?] conscience; and yet believed it. And I continued to uphold it for many years, nor do I think that it is wholly shaken nowadays, but rather has received fresh illustration from the modern discovery as to the origin of granite...'<sup>1</sup>

In 1854 he wrote of his 1822-4 trips through the Lake district:

'I had good eyes when I worked Lakeland; but at that time we knew not of the Old Red fishes; and I therefore never looked for them... No fossils have, as yet, been seen in the slates &c which alternate with the porphyries between the Skiddaw slate and the Coniston Limestone; but if you cross them keep your eyes open; and possibly you may find some rare fossil. For when I crossed them again and again (30 years since) I was looking for sections rather than for fossils...'<sup>2</sup>

Similarly, in 1856 he wrote:

'When you are looking for Skiddaw slate fossils... hammer well the gritty rocks which appear in several deep ravines... I never examined them for fossils in 1823 and 1824, because I foolishly thought that they were all below the region of animal life. At that time I had not quite learned to shake off the Wernerian nonsense I had been taught...'<sup>3</sup>

Clark and Hughes record a further letter to Lyell in 1845, in which Sedgwick says that in 1819 he was 'eaten up with the Wernerian notions - ready to sacrifice my senses to that creed - a Wernerian slave.'<sup>4</sup>

Sedgwick's recollections are not very reliable in some ways. In this instance (and as we shall see it was a pattern followed elsewhere) he does not blame his Baconian assumptions. Fish scales in rocks are by no means obvious. A part of geological field work - especially in old rocks - involves 'seeing' sometimes ambiguous patterns as particular objects because of a geologist's training. Observation itself is theory-laden. But Sedgwick never ever really grasped this point (even though it is certainly there in his friend Whewell's philosophy of science). Thus, whenever he looks back on a past failure of induction, instead of admitting any problem in the Baconian concept of induction from 'facts', he rationalises that the method was improperly applied. This is the first of three major examples we shall consider. Simple Baconian induction would imply that any impartial observer would see fossils. Sedgwick manifestly did not, so he 'post-rationalises' that he was 'ready to sacrifice his senses to the Wernerian creed'. His senses (as any good Baconian would know) would clearly have told him that there were fossils - but he ignored his senses because of his creed.

A further point from the above is that he states that he was 'looking for sections rather than fossils'. This needs noting in the context of his attitude to Smithian ideas. Some further general comment on this may be made here. Almost the first geological work Sedgwick undertook was in 1819 around Bristol and in the West country. He did so for part of the time in company with a certain Dr W.H.Gilby, who had already published papers on the

geology of the areas. In one of these, in 1814, Dr Gilby wrote to the Editor:

'Some time after I had commenced my inquiries, I saw for the first time Mr Townsend's very valuable book 'On the Character of Moses established for Veracity as a Historian,' wherein I found interspersed throughout the body of the work much important information respecting the structure of this quarter...'<sup>5</sup>

His object, he says, is to supplement this. In spite of its improbable title, Townsend's book was later called by Woodward 'the best English work on stratigraphical and topographical geology' at the time of its publication. It is known for bringing to light much of Smith's work. Gilby, then, cites Townsend, and also cites Farey who was another leading Smithian. Gilby's article uses the terms 'lias' and 'oolite' which they used. Paradoxically, however, he says nothing about fossils until the very last paragraph, when he adds:

'Organic Remains After the very elaborate manner in which Mr Townsend has treated this subject, I should only be repeating his observations were I to attempt to add anything...'

Yet the lithological details are listed, and this shows the kind of geological environment in which Sedgwick first met the serious study to be his life's work.

Others at that time recognised the particular approach which might be expected from a Cambridge Mathematics Tutor. Thus when J.J.Conybeare (brother to W.D. and one of the Buckland circle) met him in 1819 he wrote soon after:

'Having been for some time head Mathematical Tutor of Trinity he brings to the study of Geology all the subsidia that a thorough knowledge of mathematics and natural philosophy can give him.'<sup>6</sup>

A most interesting retrospect on Sedgwick was given as an obituary by John Phillips. Phillips was the nephew and early helper of William Smith, mixing in scientific circles at the time of Sedgwick's appointment in 1818. Of that period he wrote:

'At this time the importance of organic remains in geological reasoning, as taught by Smith, was not much felt at Cambridge, where a new born mathematical power opened out into various lines of physical research, and encouraged a more scientific aspect of mineralogy, and a tendency to consider the phenomena of earth structure in the light of mechanical philosophy. This is very apparent in the early volumes of the Cambridge Philosophical Society, established in 1819, with Sedgwick and Lee as secretaries. Accordingly, the earliest memoirs of Sedgwick, which appear in the Cambridge Transactions for 1820-21, are devoted to unravel the complicated phenomena of the granite, killas and serpentine in Cornwall and Devon...'<sup>7</sup>

Phillips was corresponding with Sedgwick over their geological papers in the late 1820's, and sent historical information to Sedgwick in 1831 concerning Smith to whom Sedgwick was shortly to present the Wollaston medal. Their correspondence continued as Phillips rose to eminence as a Professor of Geology, and Sedgwick's many letters to him during the Murchison controversy are in the Ashmolean in Oxford. Phillips' comments on Smith are not necessarily always to be taken at face value (he would naturally wish to amplify his uncle's originality and importance). Nevertheless, his comments on Sedgwick do have a unique significance. Had Phillips been able to claim that Sedgwick 'early grasped the key teaching given the world by Smith' he would surely have wished to do so in his obituary. But, although Sedgwick did not deny a place to fossils, what Phillips in fact emphasizes is the mathematical

approach of Sedgwick to his science - and that he was 'devoted to the Newtonian philosophy'.

### 7.3.2 Sedgwick's 1821 Syllabus

We have some more direct evidence of Sedgwick's approach at that early period from copies of his Syllabus of a Course of Lectures published in 1821. This was printed with blank pages for notes, and a copy of the Syllabus with Sedgwick's own notes is in the Sedgwick Museum at Cambridge. The University also has a copy with notes by a student A. Patterson of St Johns. No dates are given, but Patterson's copy may be fairly reliably dated. One of the books recommended in the notes was 'Miller's new work'. This must refer to J.S. Miller's A natural history of the Crinoidea published in 1821. It also says:

'Of the books that Professor Sedgwick recommended to those who are desirous to become Geologists - There was a small work of Phillips [sic] called "Facts concerning the Geology of England" a very good work: partly written by Mr Conybeare...'

This is actually confusing, but makes no clear reference to Conybeare and Phillips' 1822 Outlines of the geology of England and Wales.<sup>8</sup> Parkinson's 1804-1811 works are mentioned but not his 1822 Outlines of Oryctology; an introduction to the study of fossil organic remains. This, with reference to Miller's 'new' work of 1821, places the notes around 1822.

Sedgwick actually began (as do many lecturers) with a book list. For our purposes, however, we may begin with the general introduction which he gives. For simplicity of presentation I will put all quotations from the actual printed syllabus in capital letters. Thus he begins:

'GEOLOGY - ITS OBJECTS - PRACTICAL AND SPECULATIVE'

Patterson's note appears around this point:

'...as the speculative part of Geology, the less that is said the better; [? illegible] are too much accustomed to speculate and this is a dangerous subject to speculate on. rather let us be practical Geologists more especially as so much to be done in Geology.'

We note that Sedgwick here sees speculation and 'practical' work as separate, and whilst not saying that speculation is to be always forbidden emphasizes the enormity of the practical task first.

'2. MATERIALS COMPOSING THE SURFACE OF THE EARTH - ORGANISED - UNORGANISED'

Patterson's note is:

'All physical knowledge must be referred to the surface of the earth - important therefore to become acquainted with the surface of the earth - now Geology is the account of the structure of the earth with regard to mineral masses their structure, formation and relative situation as to the surface of the earth.'

'Geology embraces ~~of~~ the study of the globe in general and of the various relations that the different masses of which it is composed to each other [sic]. Mineralogy is as it were the alphabet of Geology. The globe we inhabit is 8000 miles in diameter & 25000 in circumference. Its surface has two grand divisions land and water. Geology may be said to embrace little more than an enquiry into the ~~present~~ history & ~~surface~~ present state of the surface or crust of the globe.'<sup>9</sup>

Sedgwick's own notes are much more sketchy (as one might expect) but on this he states that geology:

'makes us acquainted w<sup>t</sup> the structures, relative situation & mode of formation of the great mineral masses w<sup>h</sup> compose the crust of the ~~⊕~~ - Jam: Geol.'

A number of points here are of interest. First, though Patterson's notes first state it in terms of structure, they then add that it is a study of its 'history and present state'. The time dimension is introduced. Secondly, Sedgwick's notes refer to the formation of the mineral masses - so he is interested in the causal mechanisms. Thirdly, the subject is introduced as a study of 'mineral masses' - not of 'strata'. Perhaps in an introductory lecture Sedgwick wished to use language they understood, but it is significant that mineralogy is seen as the 'alphabet', and fossils are not even mentioned.

'3. DIVISION OF PHYSICAL SCIENCES - NATURAL PHILOSOPHY -  
NATURAL HISTORY.'

Patterson's note reads:

'In natural Philosophy we become acquainted with the laws of nature - we see things & often are able to imitate them. but in Natural history we cannot do this - here we see physical materials & the vital principles - now Geology is a branch of Natural History -'

Sedgwick's note at this point is partly illegible, and no clear idea emerges of the implications of the distinction as Sedgwick taught it.

The next few parts may be taken together:

'4. ORDER IN WHICH THE PHYSICAL SCIENCES HAVE BEEN STUDIED  
NOT THE RESULT OF ACCIDENT.

5. ANCIENT SPECULATIONS ON THE THEORY OF THE EARTH INADEQUATE,  
BECAUSE FORMED

(1) WITHOUT AN EXTENDED ACQUAINTANCE WITH THE LAWS OF NATURE

(2) WITHOUT ANY KNOWLEDGE OF THE INTERIOR STRUCTURE OF THE  
EARTH

6. INDUCTIVE PHILOSOPHY - ESTABLISHMENT OF THE PRINCIPLES OF

## NATURAL AND CHEMICAL PHILOSOPHY.'

Patterson's note reads:

'Geology as defined above not known to the Ancients- Intellectual knowledge preceded physical knowledge - and this knowledge is to a degree detrimental to physical. Among the Grks the former was in vogue, the rapid deductions, the strides that fancy might make as in the one pleased them better than the facts & minute investigations that were necessary for the other - The Platonists held "that all knowledge consisted in Generals" with such a dogma impossible for them to know much. It is necessary to know things, to distinguish separate things, to obtain facts & from individual facts & circumstances acquire knowledge - The night of darkness seems to have had this good effect that on the revival of learning men learnt to reason from facts not from theories - Bacon began this new order of reasoning & Newton con.....'

Sedgwick's notes read:

'(4) No attempt at y<sup>e</sup> Hist<sup>y</sup> of Geology  
accord to Def<sup>n</sup> . only of Y<sup>e</sup> present day -  
 But necessarily a late science.  
order 1<sup>st</sup> Study of Nature from reality  
 2<sup>d</sup> Speculation. soon carried to perfection -  
Greeks excelled in the 2<sup>d</sup>. but the dog-  
-mas of Y<sup>e</sup> schools unfavourable  
 to induction in physics.\*



\* Thus Y<sup>e</sup> Platonists held that  
all real knowledge was conver-  
- sant with general truths, &  
that particular phenomena could  
only be properly understood as conse-  
quences of the general

[This note on a  
separate page -  
starred as shown]

(5) Speculations inadequate

But they prove that traditions  
were extant agreeing w.<sup>h</sup> our sacred  
records -

(6) Night of Y<sup>e</sup> human understanding &

sleep of science - Revival under

Bacon - & perfection under Newton

Theory of universal gravitation a

Generalisation

Corresponding advance in Chemical

Philosophy 1<sup>st</sup> dreams of the

Alchemists . 2<sup>d</sup> Chemical knowledge

founded in experimental induction.

all Y<sup>e</sup> [?] on Y<sup>e</sup> ⊕ composed of a few ele-

- ments - .

These combined according to fixed laws

laws - Definite .<sup>s</sup> - X<sup>lim</sup> aggreg<sup>sn</sup>. [?]

analysis & synthesis . The latter

explains the operations which have

(7) Hence the 3 conclusions stated as

examples

Hence Geology late in order - n of Y<sup>e</sup>

less interesting - perfection of science

for a few(?) -

Retarding causes - Sacred Hypotheses

late also 'Y<sup>e</sup> physical facts widely scattered

((9) Order of strata - appearance of

organic remains in them - diluvium

quote D' Aubisson p.33)

and not to be easily examined except

in cultivated & civilised countries.<sup>10</sup>

This gives us quite a good picture of what Sedgwick must have said.

Early Greek Platonism is caricatured as a kind of rationalistic

speculation. There are, of course, various connections which

can be made between early Platonism and Newton, the most obvious

being the seeing of nature in mathematical terms. Plato

did not expect the actual physical world to totally conform to

perfect mathematical laws - and a parallel might be drawn to

the vacuous truth of Newton's abstracted laws.<sup>11</sup> It would be

foolish to press these comparisons too far, or to deny the real

differences between Newtonian science and Platonism. But the

point being made here is that Sedgwick sees Newton through

particularly 'Baconian' eyes. The adequacy or inadequacy of his

actual treatment of the Platonist philosophy is of less interest

to us than the implications his view of it had for his own philosophy.

The Platonic doctrine of ideal forms is very much related to the

understanding that in order to observe at all the mind must be

able to form general concepts. Sedgwick shows no grasp of this at

all. He sees generalisation as a second step after first making

particular observations.

Sedgwick's reference to the 'night of human understanding' (which appears in Patterson's notes as well) presumably refers to the 'dark ages' (as we now call them) during which science all but disappeared in Western Europe. In Sedgwick's evidently 'whistle stop' treatment, he saw the reawakening of science as coming under Bacon. Earlier science and Cartesianism are both discounted. The key phrase is 'Revival under Bacon and perfection under Newton'. The theory of gravitation is not a 'hypothesis' in a hypothetico-deductive system but a 'generalisation', made from particular and theory-free observations. This, as has been argued earlier in this thesis, is much in line with the view Newton himself tried to take. But Sedgwick seems to miss out the other half of Newton's philosophy of the study of nature - i.e. the use of speculation not to dogmatise but to stimulate. Sedgwick did in fact have some grasp of this as appears elsewhere, but it is absent here.

Sedgwick sees this pattern as applying to other sciences. He is presumably unaware of Newton's strong alchemical ties, as the extent of these has only really become known in the present century. Sacred hypotheses are seen as actual hindrances, but the late development of geology is also explained by the difficulties of actually gathering the required inductive base.

These introductory sections of Sedgwick's syllabus and notes have shown us in general terms how he saw the science of geology in 1821, and how he related it to scientific methodology as he understood it. The views of Phillips and Conybeare are confirmed; he saw it very much in Newtonian terms and in a context of natural philosophy. We may return now to the more specific item of the books he apparently recommended and what they can tell us about his geology at that time. Sedgwick's own notes are very sketchy as one might expect:

BooksGeol<sup>y</sup>

Phillips - Cuvier - Jameson -  
 Bakewell - Greenough - Playfair  
 - De Luc - Geol Trans - McCulloch  
 Breislak D'Aubuisson -

Mineral<sup>y</sup>

Phillips - Cleaveland - Haüy

## Chemistry

Conversations -

## Fossils

Parkinson - Martin - Sowerby -

Miller.'

This, of course, does not tell us anything about what he said about each book, but the omissions may be significant. Smith is unmentioned, either his map or his Strata identified by organised fossils. Townsend's book, one of the best showing Smith's actual methods, is also unmentioned. Sedgwick, however, has not neglected fossils, for no less than four books are mentioned. Again, there is no indication that any book recommendation is later than 1822 (Buckland's Reliquiae diluvianae of 1823 for example, would surely have been mentioned at a later date ?). This may indicate the date of the notes.

Patterson's notes are more detailed, and may be taken one by one:

'Professor Sedgwick recommended the following books in the study of fossils, Parkinson's organic remains, Sowerby's conchology and Millar's [sic] new work on Zoophyta, particularly Enchrinites.'

Parkinson's Organic remains of a former world was published in three volumes, 1804-1811. During this period the work of Smith

and Farey (who refers to his approach as 'Smithian') were becoming known, and by the third volume these views are expounded with appropriate acknowledgement. Sowerby's Mineral conchology of Great Britain (1812-26) used Linnaean names and Challinor has said of it that these were 'beautifully and accurately figured and not inadequately described.'<sup>12</sup> Martin's Petrificata Derbiensia; or figures and descriptions of petrifications collected in Derbyshire and Outlines of an attempt to establish a knowledge of extraneous fossils on scientific principles (both 1809) are unmentioned by Patterson though one or other or both must have been referred to as Martin's name appears in Sedgwick's notes. One is an engraved volume on Derbyshire fossils classified on Linnaean principles, and the other a book on principles of palaeontology. J.S. Miller's Natural History of the Crinoidea (1821) was a pioneering treatment of a particular group of fossils using lithographed reproductions.

Though it has been claimed in a study by David Knight that 'Careful illustrations of typical fossils' in particular stratum 'on the whole, came later than 1830',<sup>13</sup> nevertheless Sedgwick was making fairly full reference to illustrated works on palaeontology. Neither his Wernerianism nor his mathematical approach made him to either ignore palaeontology or deny its place in a study of geology. Phillips' comments, however, are corroborated. The most glaring omission from either Sedgwick or Patterson's lists is the work which perhaps embodied Smithian principles the most clearly. The Character of Moses established for veracity by Townsend in 1813 was a pioneering work on stratigraphical palaeontology, having 21 plates of fossils characteristic of the successive formations. Townsend had been studying the subject for forty or fifty years when introduced to William Smith and his ideas in 1799<sup>14</sup>. Convinced

of both the originality and importance of Smith's discovery, he framed his own work on Smithian principles and it became the first major printed channel for these views to be dissimulated. Sedgwick also fails to mention Smith's own works - either map or his Stratigraphical System of Organised Fossils of 1817. Some suggestions for the reasons for the omissions may be made later.

Patterson continues:

'He also recommended Macculloch's account of the Hebrides; but it is so elaborate & contains so many facts, it makes one head ache to read it - and Brisac who has written largely on volcanoes: and from that reason perhaps he is too fond of his the theory you cannot [one line cut off and illegible].'

Macculloch's A description of the western islands of Scotland including the Isle of Man was published in 1819. Challinor here agrees with Geikie in saying of this: 'few single works of descriptive geology have ever done so much to advance our knowledge of the geology of Britain.'<sup>15</sup> MacCulloch was Wernerian in sympathy, and Sedgwick claims in his autobiographical fragment to have been much influenced by him.<sup>16</sup>

The work referred to as 'Brisac' is Breislak (spelt correctly by Sedgwick - Patterson's many spelling errors on the names show that Sedgwick did not write them up as he spoke). No title given by either Sedgwick or Patterson, and Breislak's main phases of writing were 1798-1802 and 1822 (in Italian, French and German but not English). He was a plutonist, hence Sedgwick's comment: 'he is too fond of his theory'.<sup>17</sup> Breislak's pioneering claims for the igneous nature of Basalt were later accepted for the Trap rocks studied by Sedgwick in the later 1820's. But (rather

after the fashion of Koyre's comments on Newton and 'Hypotheses') 'theory' was a word Sedgwick applied to other people's assumptions. He does, however, mention the plutonist work.

Patterson continues:

'Of the Books that Professor Sedgwick recommended to those who are desirous to become Geologists - There was a small work of Phillips called "Facts concerning the Geology of England" a very good work; partly written by Mr Conybeare who lives near Bristol, one of the most famous geologists living - Cuvier's geology in 4 vols - a very useful work - Jamieson has translated the preliminary discourse to this work, and this is a very good book for a beginner. Greenough's is a clever work, but his turn of mind is so sceptical that he creates so many difficulties as to frighten any one who reads him from pursuing this study: however if any one who is about to study of Geology is too fond of conjecturing & of giving loose to his imagination then let him read this book.'

Phillips' book A selection of facts from the best authorities arranged so as to form an outline of the geology of England and Wales was published in 1818. It was a pioneering attempt to gather together accounts of the geology of the parts of England and Wales so far studied. Buckland provided a table of strata, though on the lower strata it was, of course, very sketchy. The four volume work of Cuvier's must be the Recherches sur les ossements fossiles (4v Published Paris 1812). This contained Cuvier's completed theory of the earth in its Discours preliminaire which was translated with notes by Jameson as Essay on the Theory of the Earth (1817). Cuvier's approach to stratigraphy had been much affected by his association with Brongniart in work on the Paris basin. Rudwick

describes four important features of this work:<sup>18</sup>

- (i) it demonstrated that the time scale must be greater than previously supposed;
- (ii) the alternating strata showing marine and freshwater conditions disproved any idea of a gradually shrinking ocean;
- (iii) this alternation could be interpreted (as Cuvier tended to) as a result of a series of cataclysmic events, each marking a new epoch of animal life;
- (iv) Brongniart used fossils for the detailed correlation of strata.

On the last point Rudwick writes that previously it had been the normal practice to use the lithology, physical position, and fossil content of a formation, with varying relative emphasès, as criteria for recognising it in widely separated areas. But Brongniart's work, according to Rudwick, demonstrated the value of precisely collected and identified fossils as criteria for tracing a detailed series of strata, which might differ little in either lithology or physical position, across an extensive area. It was the precision which was original.

Rudwick notes that Smith had used fossils similarly some years earlier, but that although Smith's work was known to English geologists its validity could not be assessed by the scientific community as a whole until some years later than Brongniart, for his map was issued in 1815 and the fossil illustrations in 1816-19. Strictly speaking Smith had priority, but Brongniart's independent discovery was first published and so had the greater influence on the direction of geological research. His method was rapidly applied elsewhere.

Rudwick's views have been given here at length because there are still questions of detail to answer about the relative influence of



Smith and Brongniart. Smith's followers (especially his nephew John Phillips) have not unnaturally wished to magnify his importance. No one needs to doubt that Smith had worked out and applied his method, essentially by himself, by 1800, and confided it to his naturalist friends Rev B. Richardson and Rev J. Townsend. But Richardson also wrote to Sedgwick in 1831:

'In consequence of Mr Smith's desire to make so valuable a scientific discovery universally known, I without reserve gave a card of the English strata to Baron Rosencrantz, Dr Muller of Cristiana, and many others in the year 1801.'<sup>19</sup>

There was always a hint of suggestion from the Smith camp that the idea (even though no details had been published) might have spread around Europe. But the first actual printed details of Smith's method were in Parkinson's third volume (1811) and Townsend's work (1813), both after Brongniart had begun to publish. In connection with Sedgwick two main points need to be made. The first concerns the presence of Cuvier rather than Smith or Townsend in his book recommendations. It is not difficult to imagine why Cuvier would appeal. Cuvier had the winning combination (as far as Sedgwick would be concerned) of a strongly structured theory whilst at the same time denying (in a way which in England would have been called Baconian) that he had anything beyond inductive observation. To Cuvier geology was 'une science positive' and throughout his life he played down theoretical ideas in favour of 'positive facts'. At the same time Cuvier and Brongniart had a wider conception of natural philosophy and the place of geology within it than did William Smith. Unlike Smith, Cuvier and Brongniart worked and wrote, like Sedgwick, as academics. To some extent, of course, the ascription of particular motives to Sedgwick must be speculative.

Yet the fact remains that he mentions Cuvier but not Smith.<sup>20</sup>

A second main point needs to be made, relating to this. Sedgwick first became acquainted with Smith (together with his nephew) in 1822.<sup>21</sup> It is not hard to imagine the reaction of Sedgwick, whose own roots were with the rugged statesmen of the Dales and whose rooms carried a picture of his old teacher John Dawson. But the personal contact appears to have nevertheless been minimal. When, in 1831, the Council of the Geological society awarded the Wollaston medal to Smith, Sedgwick knew that he would have to make a speech on the occasion as President. To prepare for this he had to write to Phillips at York early in February<sup>22</sup> asking for details of Smith's discoveries. Phillips was only too pleased to oblige<sup>23</sup>, and when Sedgwick gave his address on 18th February he had Phillips' version of the facts (relayed from Smith himself) at his fingertips. The fact that he did not already know them says something about his professional relationship with the work of Smith.

In his address Sedgwick, as might be expected, waxed eloquent on the life and work of Smith. But all that he could find to declare of his own personal debt was that he had used Smith's maps when studying the Oolitic strata in the field; he cannot refer to any theoretical influence.<sup>24</sup> Sedgwick's claims for Smith's originality and priority are correct; it is his direct influence we are questioning.

Part of what might be called 'the Smith myth' may have been helped by Sedgwick's own eulogies - even though eulogies are right and proper on such occasions. We may consider the following passage from Clark and Hughes:

'When Sedgwick began to work, geology was still in its infancy.

Until recently, theory, rather than induction based upon the observations of facts, had held undisputed sway; and, after the

publication of such works as Woodward's Theory of the Earth the rival opinions of the Wernerians and Huttonians had divided the so called geologists into opposing camps. While these profitless battles were proceeding, William Smith, whom Sedgwick rightly called "the Father of English Geology," had shown that the proper sequence of the strata might be readily ascertained by observation of the fossils characteristic of each, and that by this means the composition of the crust of the earth might be arrived at - a pursuit likely to lead to more valuable results than theories of the forces by which that composition had been moulded. This discovery - the importance of which it is difficult to realise at the present day - worked a revolution. Theory was abandoned - the mineral composition of the rocks, together with the whole science of mineralogy, ceased to be studied by geologists pure and simple - but instead a number of accurate and painstaking observers set to work in different parts of England to note the sequence of the strata, their relations to each other, and above all their characteristic fossils. Sedgwick became an ardent member of this band of explorers..<sup>25</sup>

Though Clark and Hughes admit that Sedgwick's early work was Wernerian it is dismissed as a temporary aberration. The account reflects the myths propagated so well by Lyell, and its worthlessness as a factual account is obvious even in the light of the facts presented so far in this thesis. The positive and lasting effects of Wernerianism on Sedgwick's methodology are here denied, the theory/induction distinction is simplified and greatly exaggerated, and the influence of Cuvier and Brongniart rather than Smith is completely ignored. But Clark and Hughes contribution to the myth may still have some influence.<sup>26</sup>

The other point of Cuvier's work which we know appealed to Sedgwick in this period was the catastrophism, as soon to be interpreted by Buckland. Consideration of this will be deferred until later in the present chapter.

Returning now to Patterson's notes, we see that Greenough is mentioned much as we might expect from Sedgwick: as a kind of antidote to the over theoretical, but otherwise too extreme about 'induction' for Sedgwick's taste. Sedgwick recognises, in practice, that for all his adulation of Bacon, assumptionless research into stratigraphy would simply lead to confusion in a welter of detail. With Whiggish hindsight we might expect that a recognition of the futility of the Greenough approach would stimulate him to take a more hypothetico-deductive approach, but he apparently did not do so.

Patterson's book list concludes:

'Playfair's book is a most interesting and entertaining one. he is a great Huttonian and the grace and elegance of the language in which he speaks has rendered at Edinburgh the Huttonian theory quite popular. And it has done this good, it has made many think about Geology who would not otherwise have done so: Jamieson published in opposition to this he being an anti-Huttonian an elaborate dry and generally uninteresting work, valuable however for containing the dogmas of the Wernerian school of which Professor Jamieson is a most decided partisan and defends through every difficulty. The English are quite equal to the Geologists of other countries - however Duboissons & Brisacs works are valuable ones, particularly Brisacs on Volcanoes - as mineralogy & Chemistry are in a great degree connected with geology I should recommend a person to read Philips Mineralogy & the Conversations on Chemistry... [last line illegible]'

At this time Sedgwick evidently did not see himself as a Wernerian partisan. Presumably Wernerian (like 'theorist') was a word applied to others. At this time Sedgwick saw himself simply as inductive - only in retrospect did he call himself a Wernerian, though Werner is in fact referred to more than any other author in Patterson's notes.<sup>27</sup> Playfair, Jameson, Breislak and d'Aubuisson all correspond to Sedgwick's own notes, and have been mentioned earlier. Phillips' two books also correspond, and illustrate again Sedgwick's approach to geology. Various other names appear in Sedgwick's list, but as we have no way of inferring what he said about them the mere names tell us nothing. One name mentioned is that of Bakewell, of whose An introduction to geology (1813) Challinor wrote: 'the author makes clear statements on geological structures particularly on the outstanding ones of slaty cleavage and unconformity, and moreover uses modern terminology in doing so.'<sup>28</sup> Sedgwick's unpublished autobiographical fragment (quoted above) goes on further down to speak of his 1822 work in the Lakes:

'I learned the true lesson of all appearances of the origin of the rocks, often puzzling myself a long time with joints and cleavage planes... in that year, 1822, I had learned to distinguish the slanting cleavage from the joints and bedding.'

If Sedgwick's reflections are here reliable, we may perhaps see him following out in the field ideas found in Bakewell's text.

We come now to the syllabus proper. Both Sedgwick and Patterson wrote voluminous notes, and though it would be interesting to see published a complete transcript of both, considerations of space forbid anything like such an attempt here. What we may do is to consider some leading points. The style of both sets of notes is concrete, containing many examples (not given, of course, in the printed

Syllabus). The general structure of the syllabus is as follows:<sup>29</sup>

- (1) The Objectives and Nature of Geology  
Basic features of the earth to explain
- (2) Internal Structure of the Earth  
Werner's theory - advantages & disadvantages  
Elements and Minerals
- (3) The Primitive Rocks  
(Granite, Gneiss etc, all described)
- (4) Transition Rocks  
General Types  
Organic Remains - nature and types  
Descriptions of Rock Types
- (5) Secondary Rocks  
Description of Types & Organic Remains in Each
- (6) Tertiary Deposits  
Description and Organic Remains
- (7) Alluvial Formations  
1. Diluvian Detritus  
2. Detritus formed by the causes now in action
- (8) Volcanic Rocks &c  
(includes felspathic, basaltic and porphyritic lava)
- (9) Metalliferous Repositories
- (10) Theories of the Earth
  1. Theories formed without any knowledge of the Earth's structure
  2. Theories invented to explain the phenomena presented by the Earth. De Luc - Hutton - Werner - &c
  3. Sacred Theories founded on false interpretations of Scripture
  4. Woodward's theory - connected with the opinions of his own time...
  5. Conclusion

Though Sedgwick is sometimes critical of Werner the actual system is basically Wernerian. Patterson summarises:

'The oldest called the Primitive rocks are of a crystalline texture and therefore of a fluid nature: & so agree with what we might expect them to be, by considering the laws of gravitation - then we come to more mechanical deposits. Transition rocks - & here organic remains - then secondary rocks - after the transition class there are marks of some great catastrophe, of which we cannot give any account - afterwards tertiary rocks - and then generally chalk, in basins - full of organic remains...'

He has already noted that:

'The lowest & most level parts of the earth when penetrated to a great depth, exhibit nothing but horizontal strata, composed of various substances.'

Later again we read:

'Primitive rocks never contain animal or other organic remains, they are the oldest - have a crystalline appearance and are therefore chemical deposits...'

Very clearly here, Sedgwick believes that he has a good inductive basis for his conclusions about granite, gneiss, etc., and he does not see this as part of the 'Wernerian theory'. Thus in the introductory section of the printed chapter 2 we read:

'6. Theory of Werner - examples of structure agreeing with that theory - other examples at variance with it.

7. Divisions of formations - (1.) Primitive. (2.) Transition (3.) Secondary. (4.) Tertiary. (5.) Alluvial. (6.) Volcanic.'

Obviously he did not fully follow Werner - unlike Werner, for example, Sedgwick places Basalt neither in the Primitive nor the Floetz (or secondary), but deals with it only under volcanics. But equally obviously he sees the basic Wernerian arrangement (even if not all the explanations of that arrangement in terms of receding seas) as inductive. Putting together the three points:

- (a) its even layers - as one would expect from a liquid-covered sphere,
- (b) its lack of fossils,
- (c) its crystalline structure,

a primitive precipitate seems a very obvious conclusion. Sedgwick's later reference to 'Wernerian nonsense' and 'water on the brain' are what I have called a 'post-rationalisation' of the failure of his presumed inductive method.

Patterson's note near the reference to Werner's theory reads:

'A good theory must explain all the phenomena that are exhibited and at the same time ought to be agreeable to the laws of nature.'

But there is no elaboration, and this must presumably refer to the suggested mechanisms of Werner, not the basic structure.

Patterson records some of Sedgwick's comments on his catastrophism:

'Vallies excavated by causes not now in action. that at no very remote period the great catastrophe happened...'

After describing the ordinary erosion activity of water, air and volcanos, Patterson adds:

'The reason that Professor Sedgwick has dealt so long upon this part of his subject is to show the futility of j supposti... vallies are excavated by rivers - An hypo... which would at once subvert the Bible. it is hod... a false one: for there is physical proof that the world has not existed long as it now is, not above 5000 or 6000 years at most now upon Mr Playfair's hypothesis it would have taken myriads of years to have executed these excavation of vallies through rocks...'

If Patterson actually has his notes right here then this is the only instance known to me in which Sedgwick really came near to using Biblical arguments against physical hypotheses. But there is no indication of this in the corresponding printed syllabus:



- '8. Vallies not excavated by the rivers, Inference established by lakes, gorges, and alluvia in the principal vallies
9. On the destructive causes by which the surface of the earth is principally affected. The time during which these causes have been in action.'

Sedgwick's notes go all through these , referring to examples (e.g. Lake Geneva) concluding that no existing causes are adequate to explain the pattern '∴ Must have recourse to a catastrophe - and at no very remote period'. His notes contain no reference to the Bible.

Taken as a whole the evidence of Patterson's notes must be read as denoting Sedgwick's satisfaction that physical proof had offered independent corroboration of the Bible. It should, however, be noted that when, within a decade, Sedgwick ceased to believe in the universal flood, he did not believe that this 'at once subverted the Bible.'

In an earlier reference to the flood, Patterson's notes show a typically Sedgwickian apologetic for geology - mixing the natural theology with the pragmatic:

'The flood is well supported as to the evidence of its having taken place by the state in which we find the primitive rocks: but the latter catastrophe, be it of what nature it may, is neither as it were wholly supported to be of one kind, or contradicted of so being - Examine & establish facts then reason on the facts - Geology extends over the world - it is very extensive & a very interesting subject, it is connected with the best parts of history: it teaches us to observe how wonderful is the order which pervades all the works of the Almighty. To the scholar & gentleman it is a pursuit of considerable interest: it may arouse his curiosity and he may hope to discover something where so much

is yet to be discovered. And even to the merchant, to the  
cui bons man we have an answer as to its use - in Lincolnshire  
they are throwing pecks of gold into the earth digging for coals  
where they never can find them...'

The last point which needs to be made is that (with the exception of  
the primitive rocks in which none were to be found) Sedgwick introduced  
the idea of organic remains into each section of strata. The  
syllabus gives some indication of the detail, in particular on p.26  
in dealing with the Oolitic series he says:

'9. Organic remains - Characteristic.

(1.) Of the whole formation.

(2.) Of the several subordinate beds.'

Sedgwick's own notes show that he exhibited both rock specimens  
and fossils at this stage in his lecture.

### 7.3.3 Sedgwick and the Flood

A number of references have been made to the 'flood' in the  
context of Sedgwick's Syllabus. In 1820 Buckland had published  
his inaugural lecture for his newly established Readership in  
Geology at Oxford.<sup>31</sup> Cuvier's series of catastrophes was taken  
by Buckland as universal, and he wrote:

'Again, the grand fact of an universal deluge at no very remote  
period is proved on grounds so decisive and incontrovertible,  
that, had we never heard of such an event from Scripture, or  
any other authority, Geology of itself must have called in the  
assistance of some such catastrophe, to explain the phenomena of  
diluvian action which are universally presented to us, and which  
are unintelligible without recourse to a deluge exerting its  
ravages at a period not more ancient than that announced in the  
Book of Genesis.'<sup>32</sup>

Now Buckland did not argue his geology from Moses - he claimed that the physical evidence was independent, as the above quotation shows. In an Appendix he gave a summary of 'the Proofs afforded by Geology, of the Mosaic deluge'. In actual fact nearly all the proofs may be explained by the glaciation theory (to which Buckland quietly switched in the late 1830's)<sup>33</sup>. The actual universality of the flood was 'proved' thus:

'8. The analogous occurrence of similar phenomena in almost all the regions of the world, that have hitherto been scientifically investigated, presenting a series of facts that are uniformly consistent with the hypothesis of a contemporaneous and diluvian origin.'

This was, of course, the weak link. Few inductivists could doubt that some agency other than existing ones on the areas was needed to explain the evidence, and (without hindsight) water seemed the only logical one. But either one universal flood or a number of local ones could explain the widespread nature of the phenomena - and Buckland gives no real evidence that it was a single universal flood.

Nevertheless, it seems to have convinced Sedgwick. In 1825 he wrote a two part article for the Annals of Philosophy defending the diluvialism of Buckland. The immediate occasion for this was the attacks being made on Buckland's theory by Rev John Fleming. Fleming was at that time Church of Scotland minister in a small Fifeshire parish, and in religion associated himself with the Free Church of Scotland breakaway in 1843 led by the Evangelical Chalmers. He was also, however, recognised as an important zoologist, was at that time a disciple of Jameson, and in 1834 was given the chair of Natural Philosophy in Aberdeen. Ironically, Fleming was if anything slightly more inclined to Scriptural geology

than Buckland and Cuvier - though this may perhaps appear more so in his articles than was really the case, as a device to nullify the criticism of those who had called his view 'infidel'. His full views were actually best laid out in an article in 1826 after Sedgwick's had appeared in 1825.<sup>34</sup> Fleming was not very happy with the way in which Cuvier had treated the Mosaic account as derived from Egyptian tradition rather than revelation, nor was he convinced that all this assumption of rushing torrents which completely changed the landforms was consistent with the actual Mosaic description. Thus his article begins with his objections from Scripture - he himself advocating a gentle flood which left no trace. But the second part of the article is a frontal attack on Buckland's theory from a scientific point of view. Basically, Fleming argues that the shape of the valleys and the kinds of detritus which Buckland invokes his rushing torrents to explain are very implausibly explained by it; moreover, Fleming also questions the plausibility of Buckland's explanations of the animal remains in the Kirkdale cave.<sup>35</sup>

Sedgwick, even on the basis of Fleming's earlier more fragmentary articles, clearly saw him as the main scientific critic of Buckland. The basic thrust of Sedgwick's article is the essentially different natures of the diluvial formations (i.e. flood remains) and alluvial ones (i.e. remains of regular activities e.g. rivers). In contrast to Fleming, who after all was a fellow Wernerian, Sedgwick finds the fossil evidence of secondary importance:

'When the order of superposition has once been made out, we may then proceed to examine the zoological phenomena of each successive deposit. Before that time, organic remains, however interesting in themselves, convey little information respecting the revolutions to which the earth's surface has been subjected.'<sup>36</sup>

The point is whether alluvial always follow diluvial. The other basic methodological ~~emphasis~~ with which Sedgwick begins is that:

'The truth of any physical phenomena can only be made out by physical evidence, and no appeal ought to be made to any other authority before that evidence has been completely investigated.'

Thus it is a purely physical question (says Sedgwick) as to whether all the diluvial formations refer to one flood.

Sedgwick proceeds to look at various alluvial formations, and at those which he calls diluvial (which we would now largely refer to glaciation). He concludes:

'We may therefore conclude on an induction founded on a very wide range of consistent observations: 1. That alluvial deposits include a large class of formations which have originated in causes as are now in daily action; 2. That the same causes have acted during a long period; 3. that during that period the deposits have not been interrupted by any catastrophe which has interposed any other deposits of a distinct character; 4. That diluvial deposits possess a distinct character from the preceding class, never alternate with them, and, from their position, evidently belong to an older epoch; 5. That during the epoch in question, the diluvial gravel was produced by extraordinary inundations; 6. That the disturbing forces which produced these inundations acted upon the earth's surface after the deposition of all the regular strata with which we are acquainted.

The separation of the incoherent materials, which are heaped on the regular strata of the earth into diluvial and post-diluvial detritus, is, therefore, a natural separation, which is at once descriptive of the things designated, and founded on the constant

relations which they bear to each other. Moreover it is unconnected with any hypothesis whatsoever, and is independent of any argument drawn from the nature of the organic remains contained in different parts of the several deposits.'

Sedgwick more than hints that, unlike Fleming, he does not 'view all things through the distorting medium of a hypothesis.'<sup>37</sup>

Sedgwick rejects the suggestion of a series of partial catastrophes, such as lakes bursting their banks. This could not account for the patterns of detritus. In his second article he proceeds to a detailed examination of 'the materials which have been torn up by diluvian currents, and scattered over different parts of our island; and from the position and extent of these materials I shall endeavour to prove that they cannot be accounted for by the ordinary operation of any known physical agent.'<sup>38</sup> In his conclusion he asserts that the floods which produced the diluvial detritus swept over every part of England in an epoch posterior to the depositions of all the regular strata of the earth. 'The facts which have been detailed seem to make it probable that the floods which produced the diluvial ? gravel were sudden and transient.' On the universality of the flood he is cautious:

'In the present state of our information, we have certainly no evidence to prove that all the highest elevations of the globe were submerged by the diluvian mountains; for the form of the great mountain chains may have been produced by some more ancient catastrophe, and we have no right to assume the existence of diluvial detritus in parts of the world which have not been examined, or which are inaccessible. We have, however, direct evidence to prove, that the diluvian floods acted on some of the highest points of Europe, and it is probable also that they have

acted on some of the highest parts of Asia.<sup>39</sup>

Sedgwick sees his conclusions as without doubt 'confirming the general argument' of Buckland's schema, but adds:

'In the preceding speculations, I have carefully abstained from any allusion to the sacred records of the history of mankind; and I deny that Professor Buckland, or any other practical geologist of our time has rashly attempted to unite the speculations of his favourite science with the truths of revelation.

The authority of the sacred records has been established by a great mass of evidence at once conclusive and appropriate; but differing altogether in kind from the evidence of observation and experiment. It must, therefore, at once be rash and unphilosophical to look to the language of revelation for any direct proof of the truths of physical science. But truth must at all times be consistent with itself. The conclusions based on the authority of the sacred records may, therefore, consistently with the soundest philosophy, be compared with the conclusions established on the evidence of observation and experiment; and such conclusions, if fairly deduced, must necessarily be in accordance with each other.'

Thus the sacred records speak of a time when 'the fountains of the great deep were broken up', and 'the investigations of geology tend to prove' a great catastrophe not many thousand years ago:

'Between these conclusions, derived from sources entirely independent of each other, there is, therefore, a general coincidence which it is impossible to overlook, and the importance of which it would be most unreasonable to deny. The coincidence has not been assumed hypothetically, but has been proved legitimately, by an immense number of direct

observations conducted with indefatigable labour...'

These passages have been quoted at length verbatim, because it will be important to note later that Sedgwick is here much more cautious than might be supposed in the conclusions he draws. On the evidence he concludes that the diluvial torrents were widespread, but not necessarily universal. As far as concerns the Mosaic account he claims no more than a 'general coincidence'. The hinted conclusion that the flood was worldwide is not followed by an explicit statement. Indeed, the words in which he interprets Genesis: 'the earth's surface was submerged by the waters of a general deluge', may or may not be intended to definitely imply a literally universal flood. Nevertheless, the hint is very strong.

#### 7.3.4 Sedgwick's Early Research

During this period, of course, Sedgwick was undertaking some actual geological work. One of his interests (which we may find illuminating as it could be seen as a kind of Achilles' heel of strict Wernerianism) was in the trap rocks. Sedgwick was early convinced that at least some were definitely igneous. Conybeare and Phillips, in their standard work: The Geology of England and Wales in 1822. themselves maintained a somewhat unaligned position, but noted: 'Professor Sedgewick [sic] also has lately examined the Trap of Northumberland, and considers the evidence of its igneous formation as complete.'<sup>40</sup> In his paper of 1822, later also published in the Philosophical Magazine in 1826, Sedgwick said of the dykes under consideration: 'All these phenomena so exactly resemble the effects produced by fire that I am unable to describe them without using language which may be thought hypothetical by those who deny the igneous origin of trap dykes.'<sup>41</sup> He describes the transformation of coal to soot or cinder along points of contact, and concludes:



'the igneous origin of a large class of trap dykes seems to be established by evidence which is almost irresistible.' He adds that the dyke must have been injected from below, and was once in a fluid state. 'The materials of which it is composed are the same with those which abound in a great many varieties of recent lava.' In his conclusion Sedgwick states clearly his principles of actualism:

'By supposing such phenomena the effects of volcanic action we bring into operation no causes but those which are known to exist, and are adequate to effects even more extensive than those which have been described.'<sup>42</sup>

But this shows that as early as 1822 Sedgwick was prepared to make much more definite conclusions about igneous rocks than some of his contemporaries. Sedgwick's later claim to have been under the influence of MacCullough evidently did not extend to caution about the origins of trap rocks.<sup>43</sup>

### 7.3.5 Sedgwick, Diluvialism, Wernerianism and Theory

In the late 1820's Sedgwick evidently changed his mind on two key geological issues. These have sometimes been confused, though in fact they are separate matters. The first concerns the Wernerian interpretation of rocks like granite, gneiss, etc - which we now recognise as igneous. The second concerns the question of diluvialism, and the extent of a worldwide single flood within recent history. In practice the two questions were associated in so far as the more igneous tradition of Hutton tended to be associated with a steady state view of the earth without any catastrophes. But as ideas the issues are separate.

Sedgwick made a well known and dramatic 'recantation' as the President of the Geological Society in 1831, but his change of view

on both these issues was rather less abrupt than has sometimes been inferred. We may to some extent trace through his shifting beliefs on both issues up to that famous speech. Around (say) 1827 we find that Sedgwick (as I have shown) had already reached a definite conclusion that some at least of trap rocks and dykes were igneous - though leaving it to some extent open that others might not have been. On diluvialism he still stood as supporting Buckland, but was aware of the criticisms of such as Fleming. Prévost had also written to him in March 1828, referring to his articles on diluvialism and referring to a memoir of his own read to the Academy of Sciences in June. Prévost tells Sedgwick how he had fought against Cuvier's conclusions on his findings of mixed marine and freshwater shells in the Paris basin, and suggests other possible explanations of the observations. Prévost notes Sedgwick's explanations of the terms 'alluvial' and 'diluvial', but cannot see why local floods or various other explanations might not serve to explain the phenomena as well, and adds 'Neither do I see how always to distinguish the diluvial and the alluvial'. The work of Buckland Prévost sees as 'the pursuit of a hypothetical idea'.<sup>44</sup>

In February 1829 Sedgwick assumed the Presidency of the Geological Society for a two year term of office. By this time he had evidently suffered some change of view on diluvialism, for there was a debate on the issues between Lyell and Buckland/Conybeare. After it Lyell wrote to Fleming:

'Sedgwick, now president, closed the debate with a terribly anti-diluvialist declaration. For he has at last come round, and is as decided as you are...'<sup>45</sup>

Lyell was prone to exaggerate (few were quite as 'uniformitarian' as Fleming) but the point is made.

In June 1829 Sedgwick went to London ready to embark on a tour of the Continent with Murchison. In a letter to Whewell dated 10th October 1829 Sedgwick describes their visit to Hartz, Eisleben, Halle, etc., and proceeds:

'This is the focus of Wernerian geology, and to my infinite surprise it is the most decidedly volcanic secondary country I ever saw. The granite bursts though on one side, sends out veins, and along the whole eastern flank the secondaries are highly inclined and often absolutely vertical. Near Goslar they are absolutely heels over head. From Halle we had a long run to Berlin...'<sup>46</sup>

The idea that granite implies an early date was dead, and, as we shall see, Sedgwick's Presidential speech the following year reflected this. But Sedgwick's views on diluvialism were also evidently developing. Lyell, again, wrote to Fleming in October soon after Sedgwick's return:

'Sedgwick and Murchison are just returned, the former full of magnificent views. Throws overboard all the diluvian hypothesis; is vexed he ever lost time by such a complete humbug; says he lost two years by having also started a Wernerian. He says primary rocks are not primary, but, as Hutton supposed, some igneous, some altered secondary, Mica schist in Alps lies over organic remains...'<sup>47</sup>

Lyell again exaggerated, for Sedgwick could still call himself in some sense a diluvian later in the 1830's, but Sedgwick's rejection of the whole concept of 'primary rocks' recognisable by rock type was real enough.

Sedgwick indicated his move of position rather more soberly in his Presidential speech of 19th February the following year:

'Each succeeding year places in a stronger point of view the importance of organic remains, when we attempt to trace the various periods and revolutions in the history of the globe. Crystalline rocks are found associated with the strata of almost every age; and the constant laws of combination which have produced a certain mineral form in rocks of one era, may produce it again in another. Nearly all the modifications of structure in rocks called primary are also found in secondary formations: and among tertiary deposits we sometimes find millstone-grit, red marl, with fibrous gypsum, red conglomerates, compact, subcrystalline and oolitic limestone; in short, all the distinguishing characters of secondary formations. The great barriers, which the fancy or ingenuity of geologists has at different times set up between the mineral productions of successive periods, have been thrown down, one after the other. I do not deny the importance of mineralogical characters; I only mean to assert that, taken by themselves, they are no certain indications of the age of any deposit whatever.

In reasoning from organic remains, by the succession of large groups alone can we establish any safe induction. Positive rules founded on the presence of particular genera or species are of comparatively small value. But the mind becomes wearied and bewildered by the endless succession of individual forms, and delights to take refuge in some generalisation: and generalisations would be excellent things if we could be persuaded to part with them as easily as we form them. They might then be used like the shifting hypotheses in certain operations of exact science, by the means of which we gradually approximate nearer and nearer to the truth.<sup>48</sup>

This is a very important passage. Sedgwick expressly renounces the typically Wernerian approach to using rock type as an indication of age, though lithological and mineralogical considerations were still important for local stratification. Sedgwick is also anxious that the correlation of organic species with time not be taken too far; only large groups are a real indication.

The last part of the quotation given above shows his attitude to hypotheses to be more than pure Baconian induction. But to Sedgwick they are heuristic devices. Possibly he may be intending to use them in a similar way to Newton in the 'exact sciences', though (as we have seen) Newton himself was not altogether consistent. But Sedgwick's words reflect a very Newtonian dichotomy between 'facts' (which Newton called 'theories') and 'hypotheses' which are conjectural and should be abandoned or altered as required. Sedgwick's words about parting with them as easily as they were formed were ironical in the light of his later controversy with Murchison. As Secord has shown, throughout the earlier part of the controversy (in the 1840's) Sedgwick was wanting to show a flexibility and dynamism in the nomenclature of the palaeozoics which other geologists generally found unacceptable.<sup>49</sup>

Earlier in the same Presidential speech Sedgwick had indicated his abandonment of the Bucklandian form of diluvialism. He first summarised the work of various individuals during the previous year on the formation of vallies. He then continues: 'If I might give my own opinion on this debated question, I should say that the existing river drainage of every physical region is a complex result, depending upon many conditions....' Evidence shows elevation in different areas to have occurred at different times, and:

'every great elevation of the land... must have produced, not merely a rush of the retiring waters of the sea, but a destruction of equilibrium among the waters of inland drainage. Effects like these must have been followed by changes in the channels of rivers, by the bursting of lakes, by great debacles, and in short by all the great phenomena of denundation. In comparing distant parts of the earth, we may therefore affirm that the periods of denundation do not belong to one, but to many successive epochs; and by parity of reasoning we may conclude that the masses of incoherent matter which lie scattered over so many parts of the surface of the earth, belong also to successive epochs, and partake of the same complexity of formation.'<sup>50</sup>

Valley formation has been complex, Sedgwick concludes, there are oceanic valleys, valleys of denundation, valleys formed by two sets of parallel elevations, and those of simple river erosion. Denundation is one mechanism explaining some features, but is no longer the single key explanation of landforms; he had ceased to believe (even if his belief had always been a strong implication rather than a statement) in a universal flood. The loss of a universal flood, of course, also meant the loss of the former strong distinction of 'alluvial' and 'diluvial' - Sedgwick now emphasizes instead the complexity of causes producing any given formation.

Before leaving the 1830 Address two further points are of interest. First, that in spite of the **emphasis in both Rudwick and Secord**<sup>51</sup>, Sedgwick used the language of time and epochs as well as that of positions of strata: Thus, for example:

'... Great lacustrine formations, of the same age with the rock-marl of the Isle of Wight are there (i.e. in Cantal and Auvergene) proved by their organic contents to have been formed and

solidified at a time anterior to the trachytic eruptions which upheaved and desolated the whole surface of the country. How long these great eruptive forces were in action it is useless to conjecture; but they were followed by ages of repose, during which the surface of the land was reformed, and deep valleys were excavated by the erosive power of water. A new period of volcanic agency succeeded, marked by domes of cinders and scoriae remaining to this day almost unchanged, and by streams of lava which may be traced from them into the existing valleys. And even these last operations, however recent in the order of geological events, were anterior to the records of history...<sup>52</sup>

Though he feels it 'useless to speculate' (in the manner of Lyell) on the actual time periods, the dimension of time and the inferences to causal mechanisms are both present here.

The final part of his address is an attack (already looked at in chapter 6.2) on the New System of Geology by Andrew Ure. In doing so Sedgwick also made some comment on general methodology. As already commented, Sedgwick seems particularly upset that not only is Ure one of their own number, but that he has quoted the sacred Bacon:

'This vanity merits castigation and reproof the more, as from the mischevious admixture of divine and human things, there is compounded at once a fantastical philosophy and an heretical religion.'<sup>53</sup>

Sedgwick applies the censure to Ure himself. He goes on to castigate Ure for the many wrong or out-of-date details of actual strata. It is obvious that Ure knows nothing first hand, and has copied out parts of different books put together without real understanding. But Sedgwick does not reject speculative geology as such:

'Are we then for ever to wander among the mere perplexities of details, and never hope for any system by which we may combine them ? ... I am not the advocate of any such sterile sentiment.'<sup>54</sup>

It is true that even in the 'very classification of our facts and of our phenomena' there remain difficulties requiring great industry and skill (Sedgwick has no illusions about a supposed simple Baconian 'fact-gathering'). But the guideline for this work is the rule of the 'father of physical astronomy', Newton's second rule of reasoning: 'to the natural effects of the same kind are assigned the same causes'.<sup>55</sup> Sedgwick takes this as the basis for his actualism: 'Effects similar in kind to those which are produced now, must in all former times have been produced by some corresponding power of nature.' We may note in passing that it is interesting Sedgwick never applied this rule to (say) the production of Basalt trap in the 1820's. Having decided that some was definitely igneous, he should have been less open to the idea that some might be aqueous.

Having, however, cited both of his 'guiding stars' (Bacon and Newton) Sedgwick felt secure to allow some reign to speculative geology. But 'The very commencement of the task of speculative geology requires a wide and philosophic knowledge of the physical world as it now is, and of all the great phenomena exhibited by the fragments of its former history.'

In other words, a thorough knowledge of facts must precede any speculation. But he knows where the emphasis should be:

'Let us, therefore, go on as we have begun; giving up our best efforts to the search of new facts and of new phenomena, and using them like men who have no higher passion than the love of truth.'

Just as one could read Popperian hypothetico-deductive falsificationism



into either Bacon or Newton, it is tempting to read into this more sophisticated ideas than Sedgwick in fact possessed on methodology. His comments about 'perplexities of details' comes near to recognising that observation without paradigm is at best barren and at worst impossible. It could be seen as implying that all observation must be theory laden. But in fact Sedgwick does not recognise this. He remains, finally, in the Newtonian position with a dichotomy between fact (or 'theory' and 'phenomena' in Newtonian terms) and hypotheses.

By this time, then, in 1830, Sedgwick has abandoned any Bucklandian diluvialism, (though still convinced of the diluvial origin of some local formations) and has abandoned Wernerian ideas on the origins of granite and gneiss (though remaining convinced of the importance of lithology in stratification). On February 18th 1831 he made his final Presidential Address to the Geological Society after his two years in office. We may best begin with the famous 'recantation' which refers to the diluvial issues already discussed. Sedgwick says:

'All the noble generalisations of Cuvier, and all the beautiful discoveries of Buckland, as far as they are the result of fair induction, will ever remain unshaken by the progress of discovery. It is only to theoretical opinions that my remarks have any application... diluvial gravel shot off from the flanks of a mountain chain during one period of elevation, and [sic - can ?] become so confounded with the detritus of another period, that no power on earth can separate them... there is, I think, one great negative conclusion now incontestably established - that the vast masses of diluvial gravel, scattered almost over the surface of the earth, do not belong to one violent or transitory period. It was indeed a most unwarranted conclusion when we assumed the contemporaneity of all the superficial

gravel on the earth. We saw the clearest traces of diluvial action, and we had, in our sacred **histories**, the record of a general deluge. On this double testimony it was that we gave a unity to a vast succession of phenomena, not one of which we perfectly comprehended, and under the name of diluvium, classed them all together.

To seek the light of physical truth by reasoning of this kind is, in the language of Bacon, to seek the living among the dead, and will ever end in erroneous induction. Our errors were, however, natural, and of the same kind which led many excellent observers of a former century to refer all the secondary formations of geology to the Noachian deluge. Having been myself a believer, and, to the best of my power, a propagator of what I now regard as a philosophic heresy, and having more than once been quoted for opinions I do not now maintain, I think it right, as one of my last acts before I quit this Chair, thus publically to read my recantation...<sup>56</sup>

Sedgwick proceeds to deny any contradiction between geology and Genesis, but for our present purpose the only sentence of interest is:

'...we have, at least, shown that paroxysms of internal energy, accompanied by the elevation of mountain chains and followed by mighty waves desolating whole regions of the earth were a part of the mechanism of nature... We have therefore taken away all incredibility from the fact of a recent deluge...'

These very important passages have been quoted in full to facilitate easy comparison with the earlier ones (also quoted in full above on pp. 400-401), to see how Sedgwick has again reconstructed the actual history of events in order to

disallow any suggestion of a defect in his Baconian/Newtonian methodology, and to claim rather than it was incorrectly implied. The actual wording of the two sets of passages, when juxtaposed, illustrates this clearly. Here, then, he begins by strongly contrasting 'discoveries... the result of fair induction' and 'theoretical opinions' in Cuvier and Buckland. He also implies that it was the Genesis account which (contrary to the aphorisms of the sacred Bacon!) influenced the physical theory. Yet in Buckland's work, as we have seen, the diluvial conclusions were claimed to be both inductively certain and independent of any biblical account. Sedgwick also tends to gloss over his own careful caution in concluding that the flood was universal. His former sharp division between alluvial and diluvial was not, in fact, associated with this assumption in his articles, but based (as he then supposed) on induction.

The basic point is that stratigraphical geology was inevitably paradigm-based. At one level, the phenomena of stratification, jointing and cleavage in an outcrop, are presumed to indicate a certain history for that area. Causes are deduced from effects; but this is Baconian enough, even if mistakes could be made. At another level, however, the aim is to 'map' a wide area, and this means assuming that between outcrops rock formations follow a particular pattern though it remains unseen. Finally, in trying to establish a common time horizon for rocks in widely separate geographical areas some presumptions must be made.

Sedgwick had started with an assumption that rock type implied age, and diluvium implied a single flood. This enabled rocks in widely separated areas to be correlated. By 1830 the deficiencies of both assumptions were apparent. So we find Sedgwick (as especially

seen in the 1830 Address) switching instead to an alternative assumption - the assumption that organic remains are important in correlation. It is interesting to conjecture what he might have done had this alternative not been available, but of course we have no means of knowing. But the assumption of similar organic remains implying a similar epoch remained an untestable assumption - and what is more the natural theology arguments then associated with a presumed progressive creation, form a direct analogy with the association of the Genesis flood with diluvialism. In neither case was the physical theory based on religious considerations, but in both cases they were associated after the physical theory was formed. In other words, the methodology adopted by Sedgwick in 1830, complete with assumptions, was essentially analogous to that in 1825. Had organic correlation later proven illusory Sedgwick would (presumably) again have made a 'post-rationalisation', and blamed a departure from Baconian/Newtonian principles rather than problems in them.

One other question on this may be asked: what led Sedgwick to his change of views? On Wernerianism it does not seem to be sudden. The existence throughout the period of the alternative Huttonian interpretation of the rocks as igneous must surely have been a factor, but it seems to have been Sedgwick's own observations first of the basaltic and then the granitic traps and dykes which led him to his new view. It is actually a classic case of the same observed phenomena (i.e. lack of organic remains) being explicable on two contrasting hypotheses (i.e. early pre-life horizon or igneous nature). But Sedgwick did not see it in these terms; rather he contrasted Wernerian delusions with good induction.

On diluvialism, following his speech of 1831 Fleming wrote to him.<sup>57</sup> He complained that though Sedgwick's views in 1831 were

similar to those he himself (Fleming) proposed in 1824:

'Yet in the Address there is not the slightest allusion to the accuracy of my earlier views, or the keen opposition which I experienced when avowing them, even from yourself - though the 'noble generalisations' of a Cuvier and the 'beautiful discoveries' of Buckland be not overlooked.'

Fleming proceeded to point out that Sedgwick was now calling 'shifting hypotheses' what seven years earlier he had said were 'completely demonstrated'.

Sedgwick evidently did not like this very much, for a second letter from Fleming several weeks later<sup>58</sup> says that he was 'annoyed by perceiving from the style of your letter that my communication had produced a greater degree of excitement in your mind than I had anticipated.' He also adds:

'I can easily conceive, as you have stated, that your change of opinion was not produced by any "facts or reasonings" of mine, yet I feel myself entitled to credit for the efforts which I made to stem the current of error... You may have approached the Temple of truth by a path of your own, and regardless altogether of the hints I had offered on the subject, yet it cannot be denied that I had reached the porch before you, and even furnished you with the means of profiting by my experience...'

Around the same time Sedgwick wrote to Murchison:

'... it was not by any of the writings of Fleming but by my own ... In speaking of my change of mind (which I was anxious to do in consequence of having been quoted by Brongniart and others as a diluvialist) would it not have been out of all keeping for me to have given a list of those who never embraced the theory - Fleming was not solitary in his opposition to

the doctrine. Humboldt ridiculed... Prévost lectured against it...

... So old Fleming is in a fury with me! This does a little surprise me. The possibility of his taking offence never entered my head for an instant... If I have been converted in part from the diluvian theory (which by the way I never held to the same extent with Buckland, as you may see if you read the last page of the only paper I ever wrote on the subject) it was... by my own gradual improved experience, and by communicating with those about me. Perhaps I may date my change of mind (at least in part) from our journey in the Highlands, where there are so many indications of local diluvial operations...<sup>59</sup>

Sedgwick here is dating his change of mind ('at least in part') as early as 1827. The Prévost correspondence raised some ideas on the complexity of valley formation, and alternative explanations of valleys, which resemble some of Sedgwick's own comments of 1830. The final stage to which he reached was not a uniformitarianism of Fleming or Lyell, for even the Noachic flood was still associated by him with rushing torrents in a catastrophe (unlike Fleming who objected theologically to such an arrangement and thought the flood gentle). Perhaps, then, as Sedgwick indicates, the process was gradual, and attributable to no single cause, as his change of view progressed. The Murchison letter does at least explain why so late as 1831 - and two years after his first statements against Bucklandian diluvialism - he found it necessary to make a public recantation. It seems the news of his changed view had not clearly reached all the continental geologists such as Brongniart, and he wished to make the break absolutely clear so as not to be quoted for

views he no longer held. But the change of view was already public by 1830, and this is the most convenient stopping point to distinguish his early from later work. All such points are merely 'bench marks', but useful none the less. His attitude to Lyell and Elie de Beaumont's respective theories are also dealt with in the 1831 Address, but properly belong to attitudes in the second phase of his work.

Before leaving the early work of Sedgwick one final point may be made. In the earlier parts of this thesis we considered the suggestion of a link in a 'Cambridge Network' of the Sedgwick circle and that of Coleridge. The question arises as to how far the approach to methodology of science in Sedgwick resembles that of Coleridge. The answer is that it does not resemble it much at all, but the discussion of this will be left to the next section when we may utilise the richer material of Sedgwick's Discourse on the Studies of the University to supplement the attitudes already established. On the basic issues of methodology, Bacon and Newton, Sedgwick always reaffirmed his commitment. As we have seen, the mistakes into which he fell were later 'rationalised' as mistakes in application of the method rather than any deficiency of the method itself.

### 7.3 Notes

1. The fragment is in the Cambridge University Library, apparently written around 1865. What the 'fresh discovery about granite' signifies is not elaborated. Sedgwick did not, in fact, publish any paper in 1819, but his memory of such historical detail is often suspect. James A. Secord, in his recent PhD Thesis on the Sedgwick Murchison controversy (1981), has drawn attention to the considerable unreliability of Sedgwick's historical accounts of his early work.
2. Letter to P.B. Brodie, September 10th 1854, in Clark & Hughes, 1, p. 250. Fleming was apparently the first to see fish in the Old Red.
3. Letter to Professor Harkness 29th August 1854, in Clark & Hughes, 1, p. 251.
4. Ibid., p. 251.
5. Phil Mag, 1814, 44, p. 241.
6. Clark & Hughes, 1, p. 213.
7. Phillips in Nature, 6th December 1873, p. 257.
8. The confusion arises because the 'small book' named was by Phillips alone in 1818. The joint 1822 work, however, was larger (D.M. Knight in Natural Science Books in English 1600-1900, p. 175, calls it the 'standard introduction to geology in the 1820's') and was not merely 'partly' but 'mostly' by Conybeare. Perhaps the joint work was so newly on the scene that Sedgwick had not yet got to grips with it.
9. These comments are actually on separate pages, but evidently refer to the same point. Perhaps Sedgwick repeated himself.



10. Even after a number of years trying to decipher Sedgwick's handwriting, it can still baffle. He also uses in these notes a shorthand:  $\oplus$  means the earth, and a  $Y^e$  has been used for a nondescript squiggle something like  $\gamma$ .
11. His first law states that 'Every body continues in its state of rest or of uniform motion in a right line, unless it is compelled to change that state by forces impressed upon it.' As has often been pointed out, according to Newton there are no such bodies, so whatever his law is, it cannot be a result of strict Baconian induction.
12. J. Challinor, The History of British Geology, a Bibliographical Study, p. 85. Challinor has also produced earlier studies, e.g. 'The beginnings of scientific palaeontology in Britain', Annals of Science, 1948, 6, 46-53 in which e.g. Parkinson and Martin are discussed.
13. D. Knight, Zoological Illustration, p. 129.
14. Phillips relates this in Memoirs of William Smith Lld (1844) p. 31 in a printed copy of a letter from Richardson to Sedgwick (dated 1831), and also in a letter of Phillips to Sedgwick (5th February 1831 in the Cambridge collection) quoting Smith to this effect.
15. Challinor (ref. 12), p. 89 was quoting from the early work Geikie, The Founders of Geology (1897).
16. See quotation above.
17. One must, of course, be cognisant here, that we are relying on the skill of Patterson at note-taking. Presumably he is summarising Sedgwick's rather more detailed comment.

18. These details are from the entry for Brongniart in the Dictionary of Scientific Biography. The entry for Cuvier is generally less helpful.
19. See Phillips (ref. 14), p. 31.
20. This is in spite of the fact that in the auction list of books after Sedgwick's death we find not only the 1821 edition of Cuvier's Ossements Fossiles, and Brongniart's 1807 Traité Élémentaire de Mineralogie, but also Smith's 1817 Stratigraphical System of Organised Fossils.
21. Clark & Hughes, 2, p. 509.
22. Sedgwick's letter to Phillips is in the Phillips collection in the University Museum, Oxford.
23. Phillips' letter (dated 5th February) is in the Cambridge Sedgwick collection.
24. His Speech is on p. 278-9 of the Geological Society Transactions. He also mentions that the terms oolite and lias passed into common use - though this again is hardly a theoretical influence.
25. Clark & Hughes, 1, p. 284.
26. Sedgwick's most recent biographer, Colin Speakman, writes (in 1982): 'However, it was not learned professors of grand theorists who had the profoundest impact in nineteenth century geology. It was a west country canal surveyor of humble origins, William Smith ... ' (p. 60). His account savours heavily of Clark & Hughes, who are quoted on p. 62.
27. Seven times in the printed syllabus, and five times in Patterson's notes in addition to these.

28. Challinor (ref. 15), p. 87.
29. All the titles except that for chapter 1 (which Sedgwick leaves untitled) are the chapter headings given by Sedgwick himself - and in chs. 7 and 10 the sub-headings are also given verbatim. Elsewhere I have summarised the contents.
30. Rudwick, The Meaning of Fossils p. 16, explains some reasons why a good inductivist would not at that time have accepted valleys as having been excavated by ordinary rivers.
31. Since Buckland often styled himself as 'Professor' this thesis has done the same - but strictly speaking he was a 'Reader'.
32. Buckland, Vindiciae Geologicae. Since we are here to speak of geological methodology and its independence from revelation, it may be apposite to note the mystery of Cannon's assertion in the Dictionary of Scientific Biography that Buckland's 'real opponent' was John Bird Sumner. Cannon says: 'By careful selection, Buckland made it appear that Sumner's position supported his own; but Buckland carefully avoided asserting physical miracle, his 'creative interference' being always a final, not an efficient cause. Buckland's insistence on the actual evidence of a deluge was partly an answer to Sumner's insistence that the Mosaic records were much more reliable than geological evidence... For years one of Buckland's roles was to keep room clear for an independent evaluation of scientific evidence within the Anglican community, in spite of increasing pressures from Evangelicalism and later, from Tractarianism.' In actual fact, however, Sumner is equally clear that 'No rational theologian will direct his hostility against any theory which, acknowledging the agency of the Creator, only attempts to point out the secondary instruments

he has employed.' (p.283). As a Christian he sees the truth of Moses as more certain than any geological hypothesis, but is clear that Moses did not mean to teach us geology, and 'the absurdity of supposing that the literal interpretation of terms in Scripture ought to interfere with the advancement of philosophical enquiry, would have been as generally forgotten as renounced, if the oppressors of Galileo had not found a place in history.' (p. 271). Sumner's position was not all that far from Buckland's - like Buckland he took Cuvier's work (or his interpretation of it) as a corroboration of Scripture (p. 275). But Sumner was in no sense a Scriptural Geologist, any more than Buckland.

33. Buckland, 'Memoir on the evidences of glaciers in Scotland and the north of England', Proc. Geol. Soc. Lon., 1838-42, 3, 332-337, also 345-8, 579-584. Martin Rudwick gives a useful brief analysis of the relationship between diluvialism, Agassiz' glacier theory, and issues of Lyellian uniformitarianism and the iceberg theory in 'The Glacial Theory', History of Science, 1970, 8, 136-157 (an Essay Review of some republished works of Agassiz). Buckland's reasons for accepting the theory are discussed on p. 151.
34. Fleming, Edinburgh Philosophical Journal, 1826, 14, 203-239.  
The theological part is pp. 203-15, the scientific on pp. 215-239.
35. This was the subject of Buckland's famous Reliquiae Diluvianae of 1823.
36. Sedgwick, 'On the Origins of Alluvial and Diluvial Formations, Annals of Philosophy', 1825, 9, p. 242.
37. Ibid., p. 250.
38. Sedgwick, 'On Diluvial Formations', Annals of Philosophy, 1825, 10, p. 21.

39. Ibid., p. 33.
40. Conybeare and Phillips, The Geology of England and Wales, p. 443.
41. The original paper, 'On the Phenomena connected with some Trap Dykes in Yorkshire and Durham', was read to the Cambridge Philosophical Society in May 1822 (based on the work of summer 1821). It was published in 1823, and later in the Philosophical Magazine, 1826, 67. The quotation is from the latter, p. 255.
42. Ibid., p. 256.
43. For MacCullough see the Dictionary of Scientific Biography and also the Dictionary of National Biography. I am unaware of any modern study of any length.
44. Prévost to Sedgwick, 26th March 1828, in the Cambridge Sedgwick Collection.
45. Life of Sir Charles Lyell, 1, p. 254, Lyell wrote similarly to Mantell.
46. This is in the Trinity Whewell collection, Sedgwick to Whewell Add. Ms A.213/20, and part is quoted in the Clark & Hughes.
47. Life of Sir Charles Lyell, 1, p. 256, Lyell to Fleming 31st October 1829.
48. Proceedings, 1830, p. 204.
49. See James A. Secord, Cambria/Siluria: The Anatomy of a Victorian Geological Debate, (Princeton 1981). (See especially chs. 6 and 7).
50. Proceedings, 1830, p. 191.
51. Rudwick's claims about the 'concrete' style of Sedgwick have been referred to in the previous chapter. Secord makes a similar background point in chapter 1 of his PhD Thesis (ref. 49).

52. Proceedings, 1830, p. 196.

53. Ibid., p. 208.

54. Ibid., p. 211.

55. Sedgwick cites it in Latin. Newton had changed this rule between the first and third editions of Principia, the third reading: '... effectum naturalium ejusdem generis eadem assignandae sunt causae, quatenus fieri potest.' (c.f., Newtonian Studies, p. 266, for changes). Sedgwick quotes it omitting the word 'assignandae' and the final qualifying three words. He does not actually use Newton's name, so presumably expected his hearers to know whom he meant.

56. Proceedings, 1831, p. 313.

57. Fleming to Sedgwick dated 15th November 1831, in the Cambridge University Sedgwick collection.

58. Fleming to Sedgwick dated 23rd December 1831, also at Cambridge.

59. Sedgwick to Murchison November 17th 1831, in the Geological Society Murchison collection, partially abstracted in Clark & Hughes, 1, p. 371.

## 7.4 Methodology and Sedgwick's Later Geology

### 7.4.1 Contexts of Sedgwick's Methodological Comments

We have seen how Sedgwick modelled his methodological ideas on Bacon and Newton, and this was repeated throughout his life. On geological paradigms, however, he underwent a major change; and we have seen how this was completed (both on diluvialism and Wernerianism) by 1830. Thus, whilst 'catastrophist' models of the development of scientific ideas are seldom appropriate and are inappropriate with Sedgwick, there is a sense in which 1830 stands as a kind of geological watershed for Sedgwick. The present chapter deals with his statements on methodology in the post 1830 period. There are four basic contexts in which he made comment on methodology. Firstly, there were comments made on the general methodology of geology. In particular, there were the issues raised by Lyell, by de Beaumont, and later by Agassiz, on uniformity, orogeny, and glacial inference. Secondly, there were comments made during the course of the work on the Cambrian system, and the unfortunate controversy which arose with Murchison over nomenclature. The work began in the early 1830's, and the controversy continued from about 1843 until Sedgwick's death. The issues here were much more specifically technical and geological, as one might expect, and in particular centred on the correct method for discovering and establishing a nomenclature for geological systems of stratigraphy. The third area in which Sedgwick made comment was that of a general description of the place and nature of science. Sedgwick's sermon which was published as A Discourse on the Studies of the University in 1833 (second and third editions 1834, fourth 1835, fifth 1850) contained much comment on these issues. Fourthly, Sedgwick

made various comments on methodology in controversy with non-geologists over basic scientific issues relating to geology. He commented on the scriptural geology (e.g. of Ure and Cockburn), on Chambers' Vestiges, and on Darwin's Origin of Species. Of these, only Darwin was 'scientifically respectable', but similar general comments were made on all three.

These four, of course, overlap, and in particular the first two may be paired (both concern geological method), and the last two also paired (the 5th Edition Discourse used a general framework to mount an attack on Chambers.)

#### 7.4.2 Sedgwick's Presidential Addresses and General Comment

Regarding 1830 as a watershed, Sedgwick's 1831 Presidential address summarized some of the ideas on which he had changed his mind prior to 1830, but also discussed new issues raised since 1830 by works of Lyell and de Beaumont. In June 1829 de Beaumont had presented to the Academie des Sciences his first ideas on tectonics, showing that ~~mountains~~ have been elevated in chains in different epochs, with each chain having a characteristic strike. Sedgwick referred enthusiastically to these new ideas in his 1831 Address, and shortly afterwards de Beaumont wrote to him expressing profuse thanks.<sup>1</sup> In May of the same year de Beaumont forwarded to de la Beche (secretary of the Geological Society) an extract of his ideas, in which Sedgwick's Address is quoted repeatedly, and de Beaumont's own theories might almost without exaggeration be viewed as an interpretation of Sedgwick's empirical researches and summaries thereof.<sup>2</sup> Lyell's first volume of the Principles of Geology was published in July 1830, and immediately achieved some popularity. The three volume set (published 1830-1833) was a general introduction to the history, methodology, and accumulated findings of geology.



A number of comments on the work of Lyell have already been made in the present thesis, but we need to remind ourselves of Sedgwick's basic objections to two ideas in the Principles and the reasons for them. These objections must be set against the background of a stated approval for 90% of the work<sup>3</sup>, and that Sedgwick evidently recommended it after 1830 in his annual course of lectures.<sup>4</sup> He objected to two basic points:

- (i) the arbitrary assumption of uniformity in levels of forces operating geologically throughout history
- (ii) the Huttonian 'steady state' view of the earth which saw literally neither trace of a beginning nor direction in the development over time of the earth.

On the former Lyell actually presented little 'evidence', but appeared to be habitually confusing the issues of uniformity and actualism. Sedgwick accepted the ~~constancy~~ of primary laws:

'I believe that the law of gravitation, the laws of atomic affinity, and, in a word, all the primary modes of material action are as immutable as the attributes of that Being from whose will they derive their only energy...'<sup>5</sup>

With this he also accepted actualistic approaches - himself coming to (e.g.) views of the igneous origins of basalt and granite by some considerations of known contemporary volcanic phenomena. But the uniformitarian assumptions he rejected:

'To assume, then, that volcanic forces have not only been called into action at all times in the natural history of the earth, but also, that in each period they have acted with equal intensity, seems to me a merely gratuitous hypothesis, unfounded on any of the great analogies of nature... The theory confounds the immutable and primary laws of matter with the mutable results arising from their irregular combination...'<sup>6</sup>

Sedgwick admits that to the Creator what we see as 'interruptions' may be necessary results of some simple laws, but to find out the scale and kinds of changes in the past we must proceed inductively.

The limits of such mutations:

'never have been and never will be fixed by any guesses of our own, or by any trains of a priori reasoning from the threshold of our argument; and the language of theory can never fall from our lips with any grace or fitness unless it appear as the simple enunciation of those general facts, with which, by observation alone, we have at length become acquainted.'<sup>7</sup>

Lyell has presented himself as the champion of Huttonian doctrines (says Sedgwick) and sometimes 'in the language of an advocate' (Lyell having recently practised at the Bar) he forgets the character of a historian. Sedgwick comments:

'An hypothesis is indeed (when we are all agreed in receiving it) an admirable means of marshalling scattered facts together, and exhibiting them in all the strength of combination.'<sup>8</sup>

But too often it can become 'like a false horizon in astronomy'. Lyell's assumption that 'the physical operations now going on, are not only the type but the measure of the intensity of the physical powers acting on the earth at all anterior periods' cannot simply be assumed a priori. If it is to be established, it is only 'by an appeal to geological phenomena', and Sedgwick finds that these in fact indicate the opposite.

Sedgwick's attitude to hypothesis is not easily characterised<sup>9</sup>, for it bears marks of basic problems in the attitudes of Bacon and Newton on whom he modelled his ideas of method. The word 'hypothesis' is used in two distinct senses: to mean an a priori assumption, and to mean the end product of an induction not yet

finally established. Newton did, of course, make a priori assumptions - but called them 'rules of reasoning'. In this connection Sedgwick is prepared to adopt an actualistic approach. But Lyell's assumption of uniformity appears to him to be purely gratuitous - and one might compare it to the Cartesian assumption that all force is impact force. It is possible but cannot be assumed. Again, analogously to Newton, Sedgwick sees the actual evidence as against the truth of uniformity (just as Newton showed that actual phenomena could not be explained by vortices).

Before turning to the positive reasons for accepting catastrophism, we may consider the second aspect of Lyell which Sedgwick found unacceptable: the steady state earth. He saw two main objections to this. The first was that the spheroidical shape of the earth indicated an originally more fluid state.<sup>10</sup> To back this up, differences between early and later fossils 'seem to indicate' a gradual cooling of the earth. This objection is ironical in the light of Rudwick's assertion that Sedgwick is a member of the 'concrete' school who 'are relatively closed to ideas from outside what they self-consciously refer to as a 'discipline'.<sup>11</sup> Sedgwick's other objection is based on the observation that 'there has been a progressive development of organic structure subservient to the purposes of life'. He applies this to the major classes of creatures rather than development within classes, but believes it refutes Lyell.

Sedgwick did not have an 'aversion' to hypotheses, but a suspicion (in the Baconian-Newtonian tradition) that they might be wrongly used in replacing or subverting observation rather than being induced from it.

Elie de Beaumont's system involved two main elements, as already mentioned. The first concerned his 'catastrophism' (he himself using the word 'catastrophe'.<sup>12</sup>). Sedgwick says:

'Every observer is aware that we often pass, without any intermediate gradations, from systems of strata which are horizontal, to other systems which are highly inclined. This is a fact independent of hypothesis; but it is now almost universally admitted, that the highly inclined strata have undergone a movement of elevation. Using then, the language of this hypothesis (to say the least of it a convenient mode of describing the phenomena) - we affirm that the inclined strata have been elevated at a time anterior to the horizontal strata which abut against them, or rest upon their edges...'<sup>13</sup>

De Beaumont spells this out even more clearly. If all the forces of nature were really progressing slowly, steadily and without change, then this kind of unconformity would not be expected. Sedgwick also believed that catastrophism was the only view able to explain both diluvial remains and erratic boulders.

This bears out his words quoted earlier; he is using a 'hypothesis' to marshal and exhibit facts in combination - the hypothesis of elevation being also universally received. But he is not gratuitously assuming it, rather he sees it as the most obvious way to unite the phenomena observed.

Similarly with de Beaumont's innovative addition to Catastrophism: the idea of mountain chain elevation. De Beaumont has shown:

'each of these great systems of mountain chains, marked on the map of Europe by given parallel lines of direction, has also a given period of elevation, limited and defined by direct

geological observations. The steps by which he reaches this noble generalization are so clear and convincing, as to be little short of physical demonstration...<sup>14</sup>

Sedgwick does not here follow any of the mechanisms suggested by de Beaumont in the development of his theory; he is speaking of the 'facts' of parallel chains and strike for contemporaneous ranges. Thus one must be careful in calling him a follower of de Beaumont. He is characteristically cautious:

'Greatly as I admire the generalisations of M. de Beaumont, they have, I think, been already pushed too far. We may follow them as our guides, but they must never take the place of direct observations...'<sup>15</sup>

How far, then, did the theory affect Sedgwick's actual fieldwork? Secord in his recent study claims that it did affect him, calling him a 'follower' of de Beaumont, and stating:

'Months before leaving for Wales in 1831 Sedgwick hailed the transformation that Elie de Beaumont's discovery of a "new faculty of induction" had worked in his geological vision. Henceforth he would take to the field with different eyes, and his field notebooks from the 1830's are filled with information on strike, dip and geological structure, precisely the information requisite for dating the rocks of Wales by the principal episodes of mountain uplift.'<sup>16</sup>

Whilst in general agreement with Secord's comment on Sedgwick's method, I believe that this over simplifies. Murchison's notebooks also contain information on strike and dip<sup>17</sup>, and their observation was a normal part of stratigraphical research irrespective of de Beaumont's theories. Sedgwick may well have had 'different eyes' but the evidence is less strong that it radically changed his actual

field practice. Sedgwick certainly emphasized the three-dimensional structural aspects of approach to stratigraphy, and may have been encouraged in this by the theories of one of his few fellow-mathematicians in geology. But this is not to say that Sedgwick presumed the theory to hold and to enable dating.

Some of the same questions remain for Sedgwick's work as for his models Bacon and Newton. Newton's strong division between phenomena and theory set against hypotheses cannot ultimately be maintained, and Sedgwick's analogous attempts to divide 'facts' from 'hypotheses' similarly exhibit problems. At what point, for example, does the presumption of fossil correlation become a 'fact of observation' rather than a hypothesis? Implicitly Sedgwick gives the same kind of answer as we found in Nagel - i.e. when it is universally received. But Wernerian perceptions of (say) granite might have been similarly universally received had Hutton not controverted them.

At this point it may also be appropriate to question some of the externalist interpretations of geology in this period. Secord again has asserted that:

'Stratigraphy... had powerful supports from the established social order of nineteenth century England: for all its internal quarrels, geology spoke to the public as a science that established order and place.'<sup>12</sup>

Presumably had the identical catastrophist theories been developed in revolutionary France they would have been seen as being supported by the revolutionaries because they pointed so graphically to the principle of the necessity of a total upheaval before 'progress' could be made. The very terms could then be read as

an aberrant projection into the scientific realm of the politics of the period: apparent catastrophes are necessary for progress. There is no doubt that Sedgwick did believe in a kind of order and harmony in the creation, and he also believed in a kind of providence in the way the world's resources were available for human use. But his philosophical roots exerted more discernable influence on his geology than did his views of the social order.<sup>19</sup>

Shortly after his Presidential Address, in 1832, the second edition of his Syllabus was published. The books he recommended in this period differ radically, as might be expected, from those previously suggested.<sup>20</sup> Bakewell is still suggested 'for a beginner', but Lyell's Principles (volume 1 especially) is at the head of the list. De la Beche (presumably A Geological Manual, 1831) is noted to be 'dry'. Jameson's version of Cuvier is listed, and so is Parkinson, and finally de Haye.

The actual syllabus begins with a general introduction, as before, but the whole Wernerian structure is, of course, gone. The separation rather than the connection of mineralogy and geology is apparent, and the second edition evidences his dissatisfaction with the very term 'Transition' though still extant in geology. The influence of Lyell's Principles may perhaps be seen in parts of the syllabus, especially the early sections, and some of Sedgwick's own notes may refer to it.<sup>21</sup>

By the third edition of his Syllabus, in 1837, the work has a decidedly modern look about it, and remained unchanged until his death. It begins, as before, with the distinction between natural history and natural philosophy, and the location of geology in the sciences. It speaks of the 'separation' of geology and

mineralogy. It is critical of such as Burnet and Woodward, noting:

'Ancient Speculations on the theory of the Earth false and defective', but goes on to speak of the 'True mode of conducting Geological Speculations.'<sup>22</sup> Sedgwick then looks at agents in the development and modification of the earth's surface. Rock types and classification of organic remains are both discussed. The strata are divided into 'Primary', 'Secondary' and 'Tertiary'. The Tertiary includes all deposits superior to the chalk. The Secondary includes an Upper Division (deposits from chalk down to lias), a Middle Division (New Red, Carboniferous, Old Red), and a Lower Division (Silurian and Cambrian). The final 'Primary Class' seems to be simply those which are below the others - but 'often metamorphic, and of very obscure relations'. Much detail is given on these sections, but chapter vi is headed:

'Successive periods of elevation - metalliferous veins - theories of the earth'

Sedgwick seems not to doubt the fact of periods of orogeny, but says:

'Geological connexions of some of the principle mountain chains of Europe - manner of investigating their several periods of elevation - sketch of the theory of M. Elie de Beaumont.'

His final point is:

'Present condition of Geology, considered both as a practical and speculative science.'

The basic point of this is that he is presenting 'speculation' (including that of de Beaumont) as logically after the inductive 'facts' earlier in the work. Needless to say, the earlier parts contain 'hypothetical' elements - such as reference to 'raised beaches' but the grander schemes are left until the end; Lyell is not mentioned by name.<sup>23</sup>



### 7.4.3 The Sedgwick-Murchison Controversy

We may now turn to consider the second main area in which Sedgwick made methodological comment: his work on the Cambrian system. Comment here will be much more brief than originally intended, as the Cambrian-Silurian controversy between Sedgwick and Murchison has recently been the subject of an exhaustive study.<sup>24</sup>

A table giving a basic framework for the issue is given in Table 7.1. Sedgwick began work in August 1831, accompanied for the first few days by the young Charles Darwin. At places their two journals may be compared, e.g.:

Darwin: 'At Henllan rock almost composed of Producti and Madreporos. Dip  $NE\frac{1}{2}N$  Half mile NW of Henllan Grey Wacke dipping N by E.'<sup>25</sup>

Sedgwick: 'Thence to Henllan; at several places the dip is about NE by N about 16 or 18 - an obscure escarpment W of Foxall etc etc. At Henllan dip =  $NE\frac{1}{2}$  point N, therefore the limestone is rounding to the W. and at the great escarpment where the Henllan rivulet joins the Elwy dip = N25E It is an ordinary Mountain Limestone and in several parts has an obscure concretionary structure.'<sup>26</sup>

Care must be taken in drawing too much from this, for Sedgwick was an established geologist whilst Darwin was really still a beginner in the subject as such. Nevertheless, the greater detail of Sedgwick is apparent, though it is Darwin who mentions the organic types. In a letter to Darwin following his appointment to the Beagle expedition, Sedgwick mentions various books. These are Daubeny (A Description of Active and Extinct Volcanoes... 1826),

d'Aubuisson, An Account of the Basalts of Saxony..., tr. 1814 (though Darwin is warned it is 'full of Wernerian nonsense'); Bakewell ('I don't think Bakewell a bad book (for a beginner)'); and Humboldt, Personal Narrative (Humboldt's journeys were not unlike those of Darwin himself in his early days). Sedgwick remarks ' - for fossil shells what is to be done' and recommends Darwin to ask Lonsdale. From this Barrett concludes:

'Sedgwick's weakness in palaeontology is shown by his inability to propose books on fossils.'<sup>27</sup>

This, however, is an exaggeration. Sedgwick was, as we have seen, recommending books on fossils to his geology classes both before and after this 1831 letter to Darwin. But Darwin has apparently enquired as to a specialist area of palaeontology, and Sedgwick therefore suggests further counsel.

Barrett's characterisation of the religious natures of the two men seem to be misinformed, and his suggestions about their personal relationship are implausible.<sup>28</sup> But the factual content of his article, and the publication of Darwin's journal for the trip are both useful. Barrett rightly makes the comment:

'A comparison of Darwin and Sedgwick's field notes and letters reveals a marked contrast in their scientific philosophies. Sedgwick's notes are mainly descriptive and in the best tradition of inductive science; they are almost entirely restricted to geological "facts". Darwin's notes are not only descriptive, but deductive and speculative. He introduces hypotheses, queries, and personal anecdotes. He draws empirical generalisations and relates broad areas of one discipline to another...'<sup>29</sup>

Care must be taken here, however, to draw the right conclusion. It

was not that Sedgwick objected to speculation or to generalisation; it was just that he saw these as separate from actual empirical observation. Thus Darwin's notes record:

'Sedgwick supposes these great valleys were caused by the rocks being stretched & the fact of there not being Limestone pebbles in the diluvium seems to countenance this.'<sup>30</sup>

But Sedgwick did not put such speculation in his field notes.

Barrett also comments<sup>31</sup> on Sedgwick's method that he had developed a system for attacking a new area: first he made a preliminary survey, getting the lay of the land, noting general topography, strike and dip, and establishing a base line of main rock types. Then he made traverses, during which detailed observations were made. Data collected during the traverses were next compiled, and profile sections finally drawn, showing anticlines, synclines, uniformities, rock layers, etc. Unlike some geologists, says Barrett, such as Murchison for example, Sedgwick did not concentrate on a single formation and follow exclusively the stratigraphic relations of that alone. We might illustrate something of what Barrett says from Sedgwick's letter to Murchison on 13th September 1831:

'I have nearly completed one base line to work upon, the rest must be done by traverses.'<sup>32</sup>

He does not say what this base line is, but on 23rd July 1832 he gives a base line thus:

'These bands of black limestone are absolutely identical with the transition lime which separates the greywacke of Westmoreland from the great system of greenslate and porphyry

of the central mountains of Cumberland. They form a very grand base line, which I have now traced from Glyn-Diffwa (five miles N.W. of Corwen) to Dinas Mowddy, a distance, as the crow flies, of about 30 miles.<sup>33</sup>

The Ashgill and Caradoc rock, correctly identified with its Lakeland equivalent, is marked on Figure 7.2. Around this time Sedgwick seems aware that it does not extend south, though he was not aware of its SW curve.

This kind of approach differs from the later approach of the Government Survey teams, which tended more to follow particular strata throughout their area. But an approach which could be used by a team of men working slowly was unsuited to a single man trying to fit geologising in between other academic and social commitments. Sedgwick's skill was remarkable<sup>34</sup> but if even the Government Survey could miss (as we shall see) an important faunal and lithological discontinuity, one need not be surprised at Sedgwick's fallibility.

It may be best to give here a brief outline (and relating it to Table 7.1) of the work and controversy which arose with Murchison. Sedgwick's early work was mainly in the N.W., whilst Murchison's was in the S.E. In 1834 they made a joint traverse together during which (apparently at Murchison's suggestion) Sedgwick reversed an earlier identification of the Meifod rocks with the Caradoc, and the friends decided that the basic pattern was for the N.W. to be older than the S.E. (as in Figure 7.3a). This was one of two basic errors which confused the early picture. But at this time, when the great thicknesses of the older rocks was known, neither of the two friends seem to have doubted that

Figure 7.2

Modern Geological  
Map Showing Sedgwick's  
Base Line.

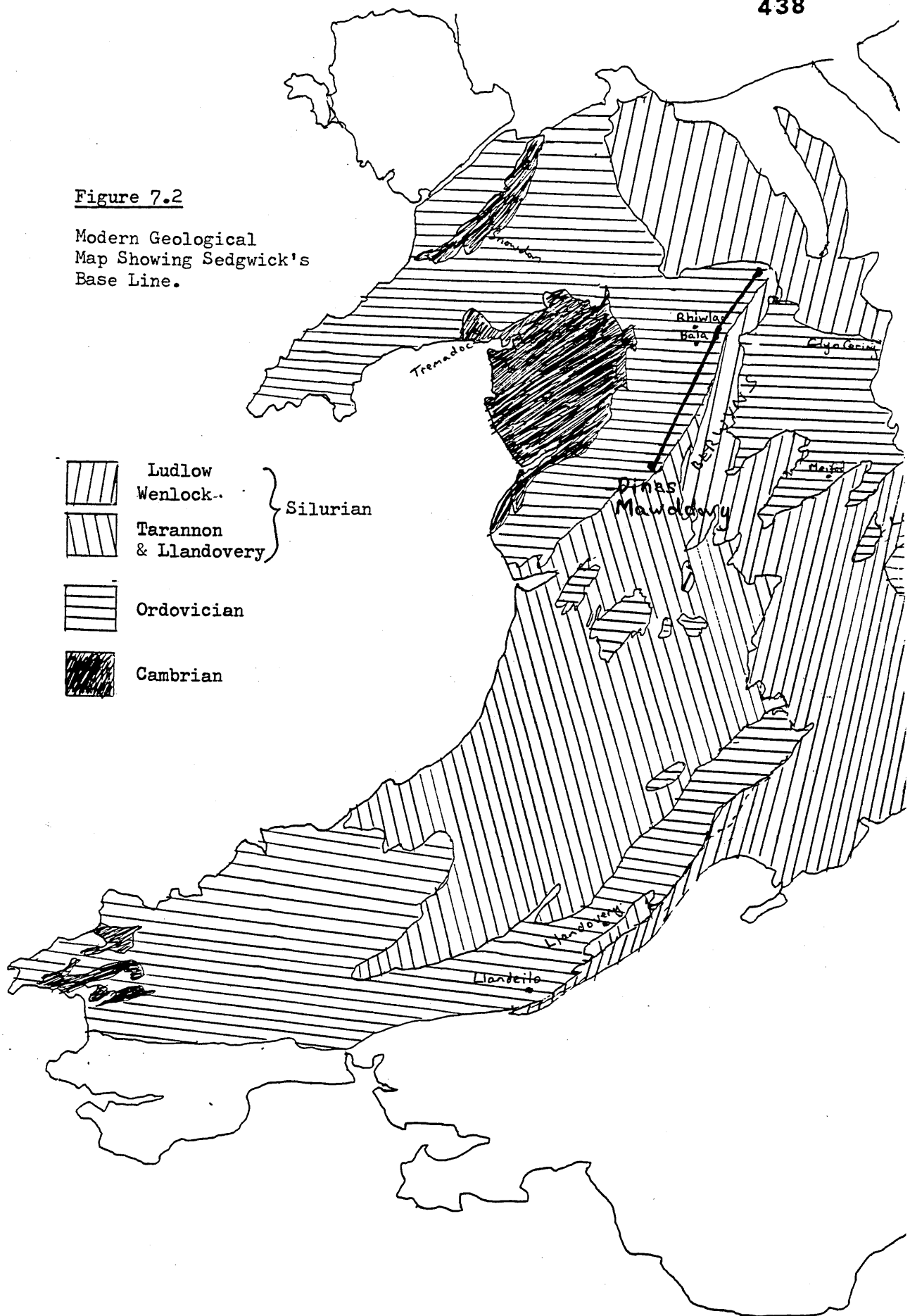


Table 7.1

Date:	SEDGWICK	Sedgwick's Method	MURCHISON	GENERAL
1831	Started work in Snowdonia		Started work in Brecon & S.	
1834	S. & M. Joint trip across contact of areas explored separately			
1837	Syllabus - two systems C & S.	distinction found lithologically	Silurian System (equal areas for Cambrian & Silurian)	
1839		- but faunal distinction never really doubted would be found		Devonian accepted as separate - above Silurian.
1840				
1841	Sedgwick's N.Wales fossils unpacked - no distinct fauna for Cambrian	flexible approach - later switched to 'protozoic'		
1843	Sedgwick's map in Geol Soc article altered (unknown to him) Silurianising whole with Protozoic = Lower Silurian	- tries limited palaeontological arguments	Murchison Silurianises most of Wales in S.D.U.K. map	
1845			The Geology of Russia - much of Russia Silurian.	
1846	Sedgwick moves (Geol Soc 16th Dec) to full Cambrian System.	Argues based on thickness of strata & geographical correctness of name		Government Survey mapping Wales area.
1852	Notes for 1st time change to 1843 map - February debate in Geol Soc Lon.	- organics subside.	over Sedgwick's article - Literary Gazette controversy	
	November - results presented showing faunal & lithological distinction between May Hill & Caradoc Sandstone.	Argues on strata organic remains and geographical names.	(Murchison previously argued increasingly just on organic criteria, now switches to established use).	



Fig 7.3c

Published under the Superintendence of the  
Society for the Diffusion of Useful Knowledge  
1843 (Dec 15th) (Chapman and Hall)

Arranged by Roderick Impey Murchison FRS  
Pres Royal Geographical Soc, FGS &c &c.

(NB THIS IS A TRACING FROM THE ORIGINAL)



- |   |                          |                                                         |                                      |            |
|---|--------------------------|---------------------------------------------------------|--------------------------------------|------------|
| 3 | <input type="checkbox"/> | Ludlow Rocks<br>Wenlock Limestone                       | } Upper                              | } SILURIAN |
| 2 | <input type="checkbox"/> | Caradoc Sandstone<br>Llandeilo Flags<br>Cambrian Slates | } Lower                              |            |
| 1 | <input type="checkbox"/> | Mica & Chlorite Slate                                   | } PRIMARY<br>(and metamorphic rocks) |            |

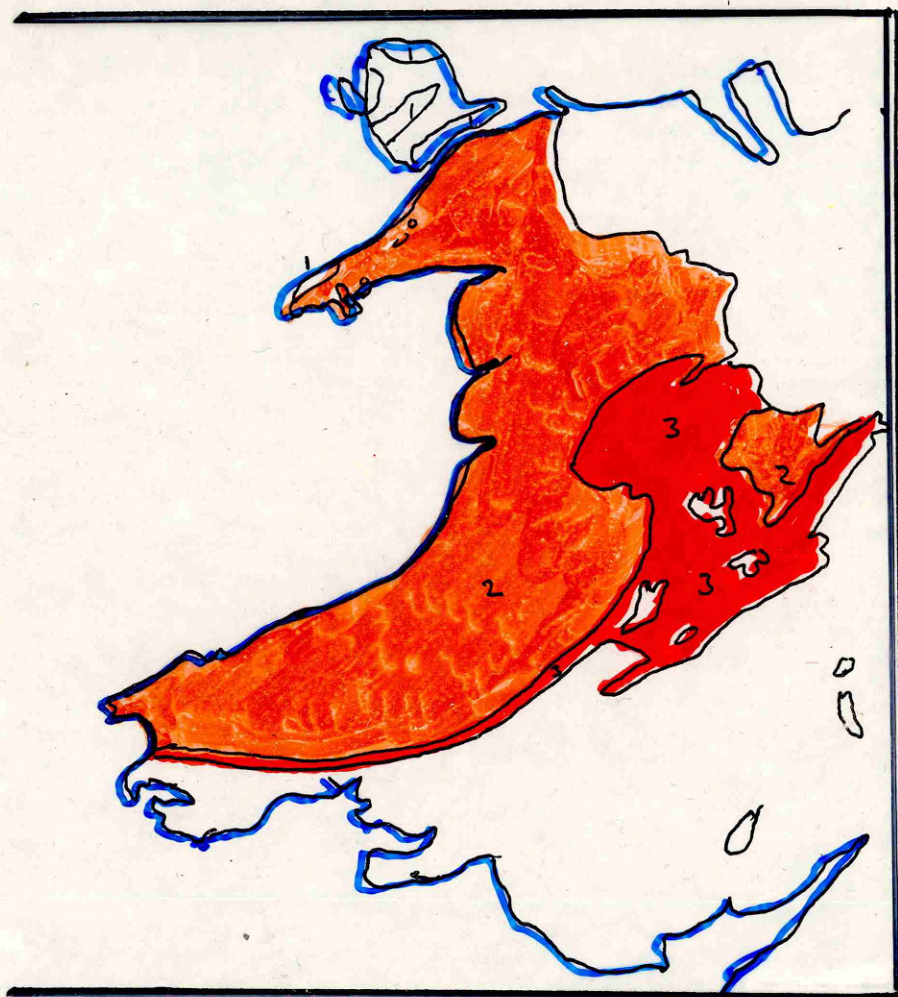


Fig 7.3c

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Society for the Diffusion of Useful Knowledge  
1843 (Dec 15th) (Chapman and Hall)

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(NB THIS IS A TRACING FROM THE ORIGINAL)






- |   |                                                                                     |                                                         |           |                         |
|---|-------------------------------------------------------------------------------------|---------------------------------------------------------|-----------|-------------------------|
| 3 |  | Ludlow Rocks<br>Wenlock Limestone                       | } Upper   | } SILURIAN              |
| 2 |  | Caradoc Sandstone<br>Llandeilo Flags<br>Cambrian Slates | } Lower   |                         |
| 1 |  | Mica & Chlorite Slate                                   | } PRIMARY | (and metamorphic rocks) |



Fig 7.3 a

Silurian System  
(1838)

- ☐ Silurian  
☐ Cambrian

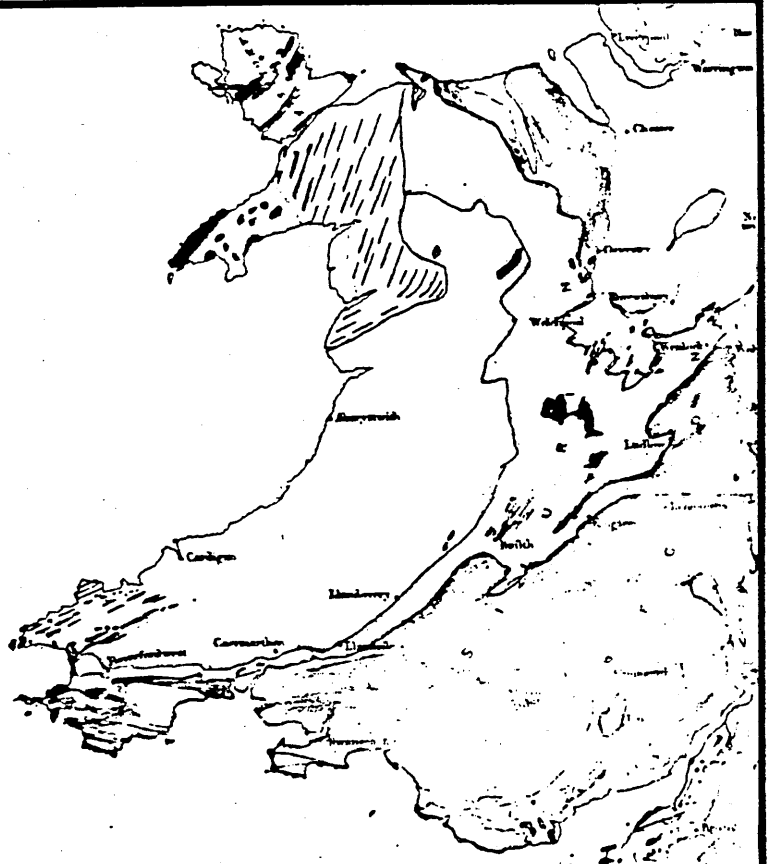


Fig 7.3 b

Siluria  
(Map 1853)

- ☐ Ludlow } Upper Silurian  
☐ Wenlock }  
☐ Caradoc } Lower Silurian  
☐ Llandeilo }  
☐ Longmynd or Bottom Rocks }  
 (Cambrian of the Government Surveyors)

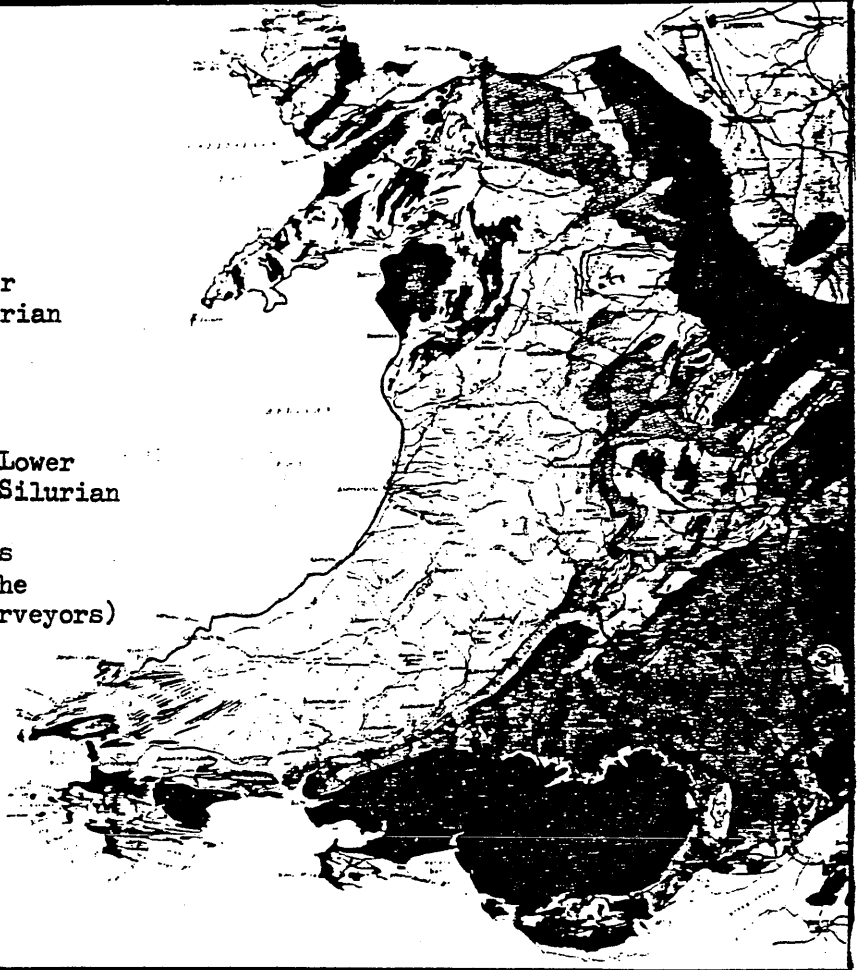




Fig 7.3 a

Silurian System  
(1838)

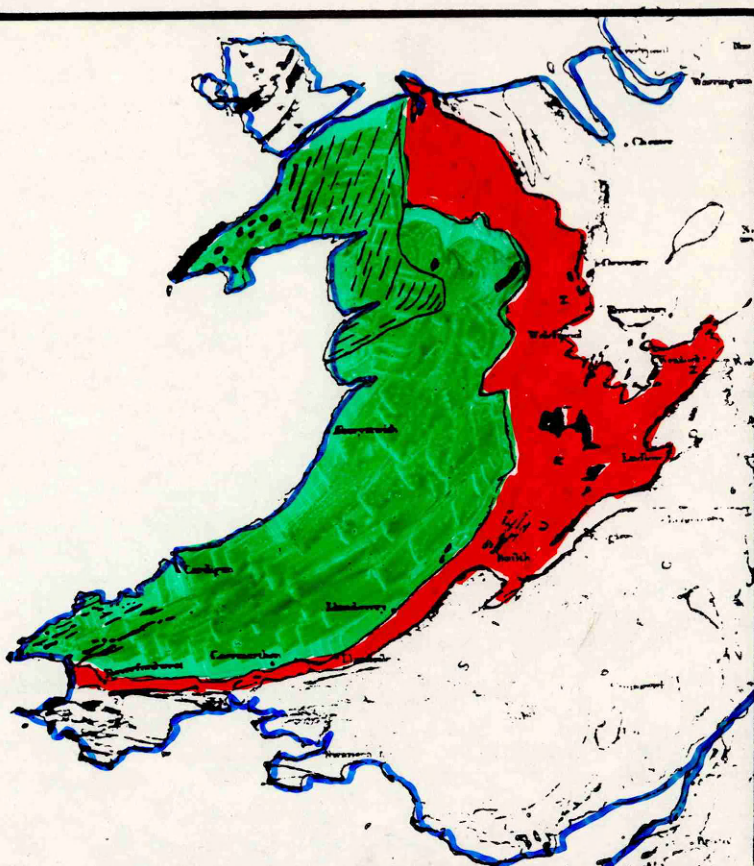
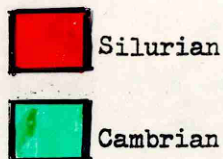
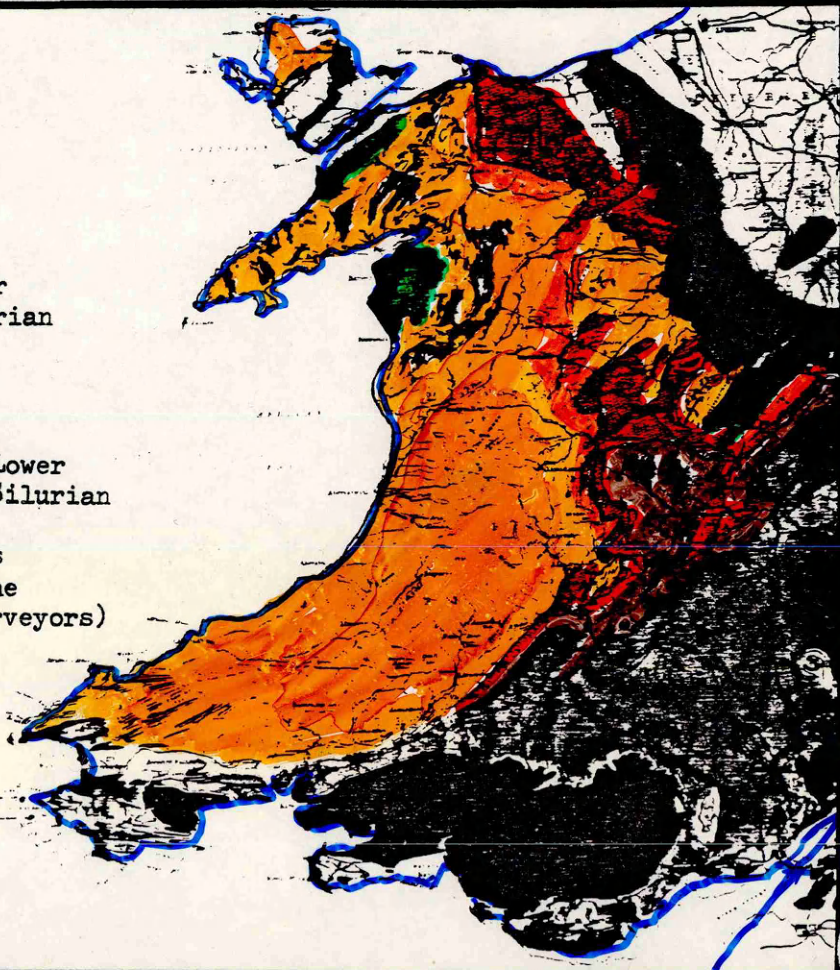
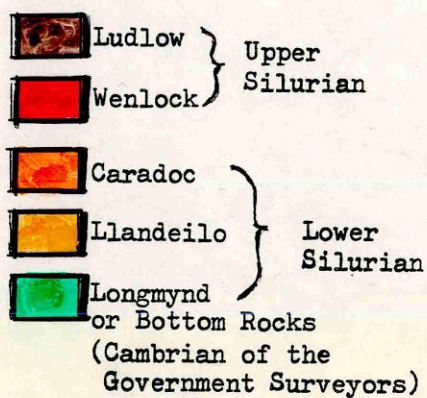


Fig 7.3 b

Siluria  
(Map 1853)



their respective systems which they named the 'Silurian' and 'Cambrian' around this time would turn out to have distinct fossil fauna. Sedgwick, during this period, did identify fossils (as his journals show) but made no systematic study of his specimens because they remained largely unpacked due to lack of suitable museum space. In the meantime, the two friends had discovered that the Devonian area (previously thought contemporary) was on a horizon above the Silurian. Thus it gave no distinct Cambrian fossils. Then, when Sedgwick's fossils began to be examined from 1841, there came the discovery that they were in fact identical to Murchison's Silurian ones: there was no distinct Cambrian set. To Murchison the issue seemed clear, without a distinct set of fossils the Cambrian system could not exist. Thus it was only rational to 'Silurianise' the whole of Wales - which he proceeded to do. Sedgwick, however, saw this as a retrograde step for a number of reasons, and adopted a flexible approach, looking for some rational system on various bases. By 1846 he had hardened to call for a distinct Cambrian system, and was still arguing for this in a debate of early 1852 at the Geological Society. By this time the Government Survey had brought to light the mistake over the Meifod rocks, which Sedgwick blamed on Murchison during their joint trip of 1834. Yet Sedgwick remained something of a singular figure in the dispute; without a distinct set of Cambrian fossils he could get little support from other geologists - however unfortunate he may have been in losing credit for his work. Then, in November 1852, Sedgwick and McCoy were able to announce to the Geological Society that during the summer they had confirmed a suspicion aroused by McCoy's examination of the fossils - that Murchison had confounded two distinct Sandstones.

Between the May Hill and the Caradoc Sandstones there was actually a lithological and fossil discontinuity. During the next few years the Government Surveyors re-examined the region, and found that the unconformity had in fact been missed throughout the whole area. Sedgwick now claimed the lower group as 'Cambrian', putting the blame for the confusion on Murchison's mistake. Murchison continued to call the whole series 'Silurian', ignoring the new break which would give Sedgwick the distinctive fossils he required. Eventually, after the death of both Sedgwick and Murchison, the suggestion of Lapworth was adopted to call the middle group by the new name 'Ordovician'. The boundaries of Ordovician/Cambrian remained a subject of dispute well into the present century.

As Rudwick and others have pointed out, the Sedgwick-Murchison controversy can be studied at a number of levels. In particular we might ask:

- (a) How did Sedgwick and Murchison respectively perceive the correct method to be for determining the superposition and nature of strata in the series ?
- (b) How were the 'facts' perceived at each stage, of the pattern of strata and of fossils. Subsidiary to this we might also ask who was responsible for the way in which the facts were perceived, and also how that perception differs from that eventually arrived at.
- (c) How did Sedgwick and Murchison at each stage regard the principles upon which a nomenclature was most rationally to be decided on the basis of the facts as perceived ?
- (d) How did each protagonist perceive his own and the other's various actions and reactions, and in what kind of personal

regard did he hold the other ?

The main points of (b) have already been outlined, and Dr Secord's thesis gives considerable detail which need not be repeated here.<sup>35</sup> In the work of the 1830's two major errors were made, for both of which Sedgwick blamed Murchison. The first was the placing of the Meifod rocks above the Bala series. The second was the confounding together of two distinct ages of sandstone: the May Hill and true Caradoc sandstone under the single title of Caradoc. The Figure 7.3 illustrates the effects of this. Murchison had taken the Caradoc area, and the Meifod and Glyn Ceiriog, as being solidly 'Silurian' strata, and the placing of the Bala series below located Sedgwick's Cambrian as a separate system. Sedgwick had, in fact, tentatively placed Bala and Meifod on the same level in 1832, and blamed Murchison in their joint trip of 1834 for the reversal of this 'true' section.<sup>36</sup> The subsequent history of changing views on these three areas (i.e. Bala, Meifod and Caradoc) is complex. In 1843 Sedgwick's view was:

'[Sedgwick] then notices the undulating country east of the Berwyns, extending from the Severn, near Pool, through the ramifications of the Vyrnwy and the Tanat. Calcareous slates, with many fossils nearly resembling those of the Bala limestone, are repeated again and again by rapid undulations... Part of the system here noticed has been described by Mr Murchison, and is classed in the Caradoc sandstone...'<sup>37</sup>

In 1846 Sedgwick asserts:

'the lower fossiliferous rocks east of the Berwyns between the Dee and the Severn - the Caradoc sandstone of the typical country of Siluria... considered as a whole their affinities

connect them more nearly with the fossils of the lower group of North Wales...'<sup>38</sup>

By February 1852 The Government Survey had shown the similarity of level in the Bala and Meifod rocks, though at that time the Bala series were still seen as below the Caradoc. So Sedgwick speaks of his reluctance in the 1830's at Murchison's identification of Caradoc and Meifod. He claims that:

'From 1834 to 1842 I had accepted Sir R.I. Murchison's conclusion, and made the Meifod beds Caradoc or Silurian, and the Bala beds Cambrian; but the only hypothesis on which this conclusion could be maintained was dissipated at the first so-called Caradoc quarry which I examined in 1842...'

The subdivision of the sections was to be decided by fossils and Salter was with him for this purpose:

'He concluded on fossil evidence, and the conclusion was borne out by the sections, that the Meifod and Glyn Ceiriog and Bala beds were nearly on one parallel. Hence, if the Meifod beds were Caradoc, the Bala beds must also be Caradoc, or very nearly on its parallel...'<sup>39</sup>

At this time Sedgwick speaks of the 'erroneous identification of the Upper Bala and Caradoc groups in 1843', the upper Bala is not (he says) Caradoc.

Secord calls this an 'elaborate technical myth' and states:

'Sedgwick reunited Bala, Glyn Ceiriog, and Meifod as the Bala beds not in 1846, but rather in the early 1850's.'<sup>40</sup> Yet Sedgwick was not a character given to direct lying, so how could this happen?

Reading the earlier articles one sees that Sedgwick does refer to the palaeontological similarities of Meifod and Bala. His retrospect in 1852 reinterprets his words 'considered as a whole



their affinities connect them more nearly' with 'the... beds were nearly on one parallel', and Sedgwick forgets that he had specifically denied in his earlier piece what now seems an 'obvious' corollary of his earlier words.<sup>41</sup> Perhaps 'myth', though, is a rather harsh description of this.

In February 1852 the Caradoc beds are placed above the Bala, though still in fact in the Cambrian system. But Sedgwick also inserts a note:

'...the Caradoc sandstone of the well known Horderley section contains numerous fossils of the Bala group, and none of the characteristic Wenlock species; while the so-called Caradoc sandstone of May Hill contains the Wenlock fossils in abundance, and none of the characteristic Cambrian types. But is there a single section in which these two distinct groups of fossils appear together at one stage? If no such section can be found, why may we not suppose that the Caradoc sandstone of May hill is a group superior to the Caradoc sandstone of Horderley? ... The statement here given is drawn from the fossil evidence supplied by the Cambridge Museum.'<sup>42</sup>

It is in the Caradoc that Sedgwick recognises the overlap of the Cambrian and Silurian (as he then conceives them) to consist. This is why he has been forced to create a 'middle group' - though on balance wants to call the Caradoc Cambrian if forced to choose. But if an actual division could be shown in the Caradoc itself 'the ambiguity referred to in the text would be at an end'. During the summer Sedgwick and McCoy visited May Hill, and in November were able to announce that the division was both lithologically and palaeontologically valid. The true Caradoc is

still placed above Bala, but is now clearly Cambrian separated by a clear unconformity from the overlying Silurian including the May Hill group.

Later, by 1854, the Government survey had shown the parallelism of the Meifod and Caradoc beds, and Sedgwick accepts this. This might seem to tell against his earlier insistence that much of the confusion arose from Murchison's wrong identification of the two. In a sense this is true, but on the other hand the division of the Caradoc into two distinct horizons had by then made the implication of such an equation (with the 'true' Caradoc) rather different.

Secord emphasizes the inconsistency and inaccuracy of the later historical reconstructions, and clearly they cannot be taken at face value. Nevertheless, usually one can see why Sedgwick in his own mind may have thought that he was correctly representing the facts.

Turning back to the question (a) concerning methodology, the geological world always recognised a difference between both the training and abilities of the two men. Sedgwick, as we have seen, started with the training of a Wernerian in mineralogy, and his tendency in this respect to structural stratigraphy would have been enhanced by his mathematical training in Cambridge. Murchison, on the other hand, was educated at a military college, and not in mathematics, and had never been a Wernerian. Soon after starting geology Murchison came under the influence of William Smith:

'From the moment I had my first walk with William Smith (then about sixty years old) I felt that he was just the man after my own heart; and he, on his part, seeing that I had, as he said,

'an eye for a country', took to me and gave me most valuable lessons. Thus he made me thoroughly acquainted with all the strata north and south of Scarborough. He afterwards accompanied me in a boat all along the coast, stopping and sleeping at Robin Hood's Bay. Not only did I then learn the exact position of the beds of poor coal which crop out in that tract of the eastern moorlands, but collecting with him the characteristic fossils from the calcareous grit down to the lias, I saw how clearly strata must alone be identified with by their fossils, inasmuch as here, instead of oolitic limestones like those of the south we had sandstones, grits, and shales, which, though closely resembling the beds of the old coal, were precise equivalents of the oolitic series of the south...<sup>43</sup>

This was in 1826, and we must be careful to see exactly what he was saying. At that time, Wernerians still identified ages of some rocks by the rock types rather than fossils, and it is to this he basically refers. Coming into a new area of similar age to one already explored, strata may be identified by the use of well defined fossil sets. Sedgwick would have agreed with this by 1831 - though would probably have put more emphasis on the use of dip and strike on a local basis even whilst using the fossils. But in any case this was not the situation in Wales. Sedgwick and Murchison were exploring areas without known age parallels having well defined fossil sets. Part of the object of the exercise was, indeed, to define such sets. Both Sedgwick and Murchison had to rely on structural three dimensional stratigraphy (using dip and strike and defining sections) at the same time as fossils, in exploring these new areas. Ironically, it was precisely

because Murchison assumed that two apparently mineralogically similar sandstones were similar in age that he confounded the two horizons of fossils from May Hill and Caradoc.

Clearly fossils cannot be used to determine the ages of rocks or levels of rocks unless the fossils typical of those ages or levels have already been defined from elsewhere; where this is not so they cannot be used as a primary factor. Sedgwick is always clear on this; e.g. in 1846:

'In every country which is not made out by reference to a pre-existing type, our first labour is that of determining the physical groups, and establishing their relations by natural sections. The labour next in order is the determination of the fossils found in the successive physical groups; and, as a matter of fact, the natural groups of fossils are generally found to be nearly co-ordinate with the physical groups - each successive group resulting from certain conditions which have modified the distribution of organic types. In the third place comes the collective arrangement of the groups into systems, or groups of a higher order.'<sup>44</sup>

This, he claims, was the approach of William Smith, and it was applied in Wales. In February 1852 Sedgwick makes a similar point in denying that Murchison has followed Smith's method:

'Smith never gave the name to a group first, and made out its place in the sections afterwards. In every instance in which he gave us geological names, his actual sections had the priority of his names by many years: and he never gave

a name to any group until he had determined its relations to the groups above it and below it... He used palaeontology as a principle of identification only where a typical group had been already well established: but palaeontology was not the foundation of his ~~nomenclature~~, for his names were local or provincial...<sup>45</sup>

By 1854 Sedgwick is in a little more 'chastened' mood, for by then it had come to light that both he and Murchison had missed unconformities in their sections:

'...in the actual state of our information, we have done well constantly to use both kinds of evidence; and on both together has our best, and our only consistent and secure nomenclature been founded. If it be true that experienced geologists have sometimes, from the neglect of palaeontology, blundered in the arrangement of physical groups, it is equally true that very expert palaeontologists, from some mistake of arrangement among the physical groups, have sometimes blundered in their palaeontology. The double kind of evidence above described, has seldom been lost sight of with impunity.'<sup>46</sup>

But he still later comments:

'... such fossils have comparatively small geological value before their real sectional position (in the general sequence of our palaeozoic rocks) has been determined; and, until that point has been cleared, they are of no value in helping us to a true nomenclature of our palaeozoic groups...'<sup>47</sup>

Sedgwick, in fact, was fairly consistent on this. Fossils were one element in working out an initial order of stratification for an epoch. Only after well established could fossil groups be the sole

means of correlating with distant areas. Murchison did not always speak as though this were so, but Sedgwick did. Perhaps, however, we may detect in the 1854 article a tendency to emphasize rather more the equality of palaeontological with physical considerations than in earlier articles. The May Hill/Caradoc episode may have illustrated this. The physical considerations perhaps raised the initial question about the ambivalent horizon of the supposed Caradoc. Yet it was McCoy's cataloguing of the fossils which first raised the serious possibility of a division. Even then, Sedgwick would conclude nothing beyond a questioning footnote until he had examined the physical sections in the field. Only when he was sure that physical and palaeontological considerations pointed to the same thing would he proceed.

In none of this, of course, does Sedgwick draw any conclusions (alien to his Baconian-Newtonian methodology) about the theory-laden nature of observation. Missed unconformities are due to carelessness or to being misled.

The third question we raised concerned the principles of 'right' nomenclature. Murchison's attitude went through three basic stages. From 1834 to 1843 he believed the Bala series to underlie the Caradoc, and never seems to have doubted that it would yield a distinct Cambrian fauna. From 1843 to 1852 he argued basically that without any distinct fauna there could be no internationally recognised Cambrian - and in any case the Silurian system was already established. After 1852, when Sedgwick had produced a potential fossil break, he argued simply from established use - both groups had been given in his original Silurian so both should stay Silurian.

To Murchison the situation in 1843 was straightforward. Soon after Sedgwick's fossils were unpacked and identified he wrote to him (in 16th October 1842):

'...But the question is, if there are no rocks containing fossils differing from those published as Lower Silurian in South Wales, are there such in N. Wales...'

In a postscript Murchison suggests that he may, with Sedgwick's permission, be prepared to write something like:

'Judging from their... great thickness and distinct lithological character it was presumed (when the Cambrian System was so named) that these greatly developed inferior slaty rocks would be found to contain a class of organic remains peculiar to themselves, the more so as the few forms then discovered in them seemed to differ from the Lower Silurian types.

Subsequent researches have however decided otherwise. In the Slaty region of the N. West of England, of which by his labour he had so long ago rendered himself the master, Professor Sedgwick has now satisfied himself that the lowest organic remains which can be traced are no others than those published as Lower Silurian whilst ... he has come to similar conclusions respecting the oldest fossiliferous tracts of N. Wales...'<sup>48</sup>

Murchison's retrospect in January 1847<sup>49</sup> presents a virtually identical picture. From his own research in Snowdon and from Sedgwick's responses<sup>50</sup>, by 1843 Murchison was sure that the supposed Cambrian region had only Lower Silurian Fossils, and so coloured the whole area as Silurian in a map of 1843. Nearly all geologists would have agreed with him - without a distinctive

group of fossils the Cambrian was doomed. However much Sedgwick might be right about fossils playing a secondary part in establishing the order of superposition in the field, few doubted that a distinct nomenclature require distinct fossils. Murchison affirms: 'No modern geologist can sustain, that peculiar lithological features or extraordinary thickness can constitute any claim for the establishment of a new nomenclature.' As for the idea of renaming the 'Lower Silurian' as Cambrian, Murchison objects that the use of 'Lower Silurian' is already well established internationally, and besides, to do so would rename large portions of the geological map.<sup>51</sup>

Sedgwick's reaction to the news in 1842-3 that there was no distinct fossil fauna for Cambrian was initially to experiment with terms like 'Protozoic', being prepared to abandon Cambrian altogether.<sup>52</sup> In 1846 Sedgwick advocated a return to use of the term Cambrian. His arguments against Murchison were two main ones. First, to call everything 'Silurian' would be to return to the old situation of calling everything 'Grauwacke' - a retrograde step. Secondly, calling everything 'Silurian' would imply that 'great groups of rocks not found in Siluria' were included in it. Distinction between the systems of 'Silurian' and 'Cambrian' is admittedly not absolute, but Sedgwick tries to develop some limited palaeontological basis based on fossil frequencies.

In February 1852 the argument is not substantially different. Again Sedgwick argues:

'... it is surely an axiom in geological nomenclature, that if we give a new geographical name to any group of strata, that name must refer us to a spot near which we find the group well



developed.'

This is, says Sedgwick, so in Cambria but not in Siluria for the oldest palaeozoic rocks. Moreover, the name also had priority. Whilst some might find the name used unimportant, Sedgwick argues that scientific names ought to be 'the **abstract** representations of the highest conceptions of the human mind' - and should follow not precede the exploration of the phenomena. Moreover the word 'system' in the strict sense is inappropriate unless a true palaeontological base is found - to construct a 'Silurian system' and then arbitrarily extend it downwards indefinitely is a bad scientific practice.

After the division of the May Hill and Caradoc in November 1852, Sedgwick continued to use the same arguments, but he now had the additional asset of a clear palaeontological and lithological break, which for the first time gave a 'true base' for the Silurian system i.e. a clearly defined starting point.

The final point (labelled (d) above) concerns the purely personal aspects of the affair. Even before the controversy, the relationship between the two men had sometimes been stormy. Sedgwick was inclined to fly off the handle if he thought that some injustice had been done - even though he later usually apologised. Unpublished letters reveal at least three little 'misunderstandings' in the later 1830's.<sup>53</sup> Now the general background to the controversy with Murchison has been well described by Secord. Sedgwick had failed to produce his big book on the Palaeozoics, whilst Murchison's works on Silurian had received much acclaim. Sedgwick had foregone any possibility of a career elsewhere, for the status offered by his Professorship

- where part of that status must inevitably be invested in his work.<sup>54</sup> Sedgwick had a general sense of injustice, but he also brooded over several particular incidents. A number of these are listed in a letter to Murchison in October 18th 1853.<sup>55</sup> The first was the shock he had when he happened to see a copy, some time after its publication, of the geological map published by the Society for the Diffusion of Useful Knowledge in 1843 under Murchison's guidance. Murchison had taken the step (which seemed only logical to him since he then knew there was no distinct Cambrian fossil set) of Silurianising most of Wales. Sedgwick's letter does not state this but we know from extant contemporary letters that Murchison had actually consulted Sedgwick at the time about areas on this map - but apparently did not inform Sedgwick of his intention to spread the Silurian colours.<sup>56</sup> A second issue was that Sharpe, in a paper of 1843, had implied that Sedgwick was to blame for wrongly assuming the Cambrian to underlie the Silurian. Sedgwick had expected Murchison as the then President of the Geological Society to set the record straight and admit the fault was his own - and Murchison had said nothing. The third issue was an error in Warburton's editing of Sedgwick's 1845 article for the Geological Society.<sup>57</sup> At the time Sedgwick was suggesting a nomenclature of Protozoic = Lower Silurian + Cambrian, but Warburton altered the map key to read 'Lower Silurian (Protozoic)' making the two equivalent terms. This had misled others into believing that Sedgwick accepted the Silurianising process - and unfortunately Sedgwick himself did not notice the alteration until 1852 on re-reading the paper. Finally, there was the blundering attempt to recall Sedgwick's February 1852 paper

from the Geological society.<sup>58</sup> Sedgwick also objected at this time to the verses by Forbes implying that Sedgwick had lagged behind.<sup>59</sup> Yet during this time Sedgwick and Murchison remained on reasonable terms. In 1854 Sedgwick had some further difficulty when he found the cuts demanded by the Geological Society in one of his papers unacceptable, and thereafter published instead in the Philosophical Magazine.<sup>60</sup> There was no break with Murchison, but Sedgwick apparently found it increasingly galling that Murchison refused to confess to what Sedgwick thought were mistakes due to him, and that he continued to try to disguise these with the device of the 'Llandovery' nomenclature.<sup>61</sup> Murchison published his Siluria in 1854. Sedgwick admitted that Murchison did not exactly claim that the mistakes of 1834 were due to Sedgwick but Sedgwick thought that he had written in such a way as to give this impression.<sup>62</sup> In Sedgwick's introduction to the Cambrian and Palaeozoic fossils (1855) he calls for the retraction of 'a misstatement injurious to myself on an unequivocal point of fact.'<sup>63</sup> Sedgwick wanted Murchison to openly admit sole responsibility for the separation of Meifod and Bala horizons. Murchison, however, was not the kind of man to whom eating humble pie in public came naturally, and in any case to his mind the separation of the two beds was simply the logical result of a fairly general belief in the early thirties that the N.W. beds were older than the S.E. In reality both men were reinterpreting actual events to be more in their own favour. Sedgwick exaggerated both his reluctance to adopt the change and the authority of Murchison, whilst Murchison preferred to obscure the issue by speaking in terms of general beliefs. Yet Sedgwick waited, in vain, for some word from Murchison. On 25th

February 1856 he wrote to Murchison:

'I continue to address you as an old friend, but this is the last time I shall have it in my power to do so, unless you fully and unequivocally retract what I have in the Introduction to the Cambrian and Paleozoic Fossils (pp. 75 and 90) called a "misstatement injurious to myself on an unequivocal point of fact." If you do not make this retraction (for which I have already waited too long) it will be impossible for me to regard you as a truth loving man...'<sup>64</sup>

In late February or early March (the letter is undated) Sedgwick wrote thanking Murchison for his reply, adding:

'The point upon which I especially insisted was this: that the order of superposition among the lower Silurian groups given in your various papers, in your Siluria map and quarto volume, was your own, and that it was not suggested by myself and adopted by you on my authority. D. Sharpe tells me now that this was the 'general belief' when he wrote his first paper, I think in 1842. This is to me passing strange for no one can know as well as yourself that from Welshpool to the W. End of Pembrokeshire, there was not one hairs breadth of the demarcation of your Silurian base, as laid down in your map, that was suggested by myself...'

Sedgwick went on to another reconstruction of events, but felt that the difference had finally been more or less patched up.

On 6th March he reread the last Murchison letter and:

'I find that yours of the 26th last month is less satisfactory than I thought it when I sent my answer...'

The blame for the mistake, says Sedgwick, was clearly Murchison's, and he should have corrected Sharpe in 1842 when he laid the blame on Sedgwick.<sup>65</sup>

Sedgwick adds that it is not enough:

'... to "Apologise to me in all sincerity for having attributed an opinion to me which I did not entertain." I did entertain an erroneous opinion but on your authority.'

Murchison has written 'NO' at the side of the last statement, and Geikie (later Murchison's apologist and disciple) claimed that 'The fact was that both men originally believed Sedgwick's Cambrian to underlie all Murchison's Silurian and it is impossible to say who first, in actual fact, came to this conclusion.'

Murchison wrote to Sedgwick on 9th March. He claimed that it would have been impossible for him in 1834 to give a definitive judgement on the contemporaneity of Bala and Meifod, as he had hardly examined the Bala area and his responsibility did not extend beyond the Glyn Ceiriog area (i.e. Meifod). He had put the relationship in the Silurian System believing it to be Sedgwick's view - but indeed that it was the general view that the N.W. was older.

Sedgwick glanced at Murchison's letter and wrote a note in which his expression of delight was sincere- but on 13th March again had second thoughts and again wrote putting sole blame on Murchison for the sections.

The relationship rumbled on uneasily in this fashion. There was a debate that year in the British Association, and later Sedgwick wrote:

'...It was his policy never to acknowledge a mistake, and on the matter of fact whether he had made a great mistake or had adopted it from me, he never spoke out till I wrung

an answer from him at the last Cheltenham meeting of the British Association. He then, at length, but far too late to save his credit as a fair dealing man, did acknowledge that the blunder, the actual inversion of the order of super-position from Denbighshire to Carmarthenshire, was his own, and not in any way borrowed from me.'<sup>66</sup>

From this time forth the estrangement between the two was personal as well as professional. Murchison would have liked to patch up the quarrel, but not at the price of admitting himself a 'great blunderer'. Sedgwick would not renew the friendship whilst he felt himself wronged by a Murchison who had neither admitted the blame for the 'key' errors (as Sedgwick saw them) nor altered his nomenclature to readmit the Cambrian. The personal dimension hinged on questions of integrity and justice, not on scientific issues.

Nevertheless scientific issues of methodology did separate the two. In one of his most perceptive passages Secord writes: 'As Woodwardian Professor of Geology at Cambridge University Sedgwick presented his subject as part of the cultural equipage of the Christian gentleman. In his view, science provided a route to moral enlightenment through the practice of a soundly based and philosophical method, and a historically just and properly founded classification -- such as he believed the Cambrian to be -- was a sign that this method had been followed... Where Sedgwick saw his Cambria in relation to the moral sciences, Murchison considered his Silurian 'territories' from his position as President of the Geographical Society... geological classification was sanctioned as much by utility and past practice as by any

philosophical justification...<sup>67</sup>

This is, perhaps, exaggerated. Sedgwick saw correct science as a way to truth and to find out more about God's world - but 'a route to moral enlightenment' is rather strong. But the Silurian nomenclature enshrined a poor scientific method in which 'systems' were defined without a proper base,<sup>68</sup> and fossils were exaggerated in importance compared with sections in defining a new system which could lead to a confusion of fossil horizons. This, at least, was how Sedgwick saw it.

We may turn now from the more technical side of Sedgwick's methodology to consider some of the comments on methodology which he made in more general contexts.

#### 7.4 Notes

1. The letter, dated 21st April 1831, is in the Sedgwick collection in the Cambridge University Library.
2. De Beaumont sent a copy of an extract from his 'Researches on some of the Revolutions which have taken place on the Surface of the Globe' to Sedgwick. It is bound (though not annotated) in one of Sedgwick's volumes which remains in the Sedgwick Museum, Cambridge.
3. Presidential Address, Proc. Geol. Soc. Lond., 1831, 1, p. 311.
4. An annotated version of the second edition, annotated by R. Birkett of Emmanuel College, is extant in the University library. The book recommendations are:
  - 'Lyell's Geology Volume 1 especially
  - De La Beche (dry)
  - Bakewell for a beginner
  - Cuvier's theory of the Q by Jamieson
  - Parkinson's Synopsis of Oryctology
  - Deshayes'
5. See ref. 3, p. 300.
6. Ibid., p. 304.
7. Ibid., p. 302.
8. Ibid., p. 303.
9. To simply say, for example, that he had an aversion to hypotheses or a reluctance to indulge in them is an oversimplification.
10. The word 'indicated' is as strong as Sedgwick himself puts it (ref. 3, p. 298).
11. See Rudwick, 'Cognitive Styles in Geology' in Essays in the Sociology of Perception, Ed. M. Douglas; though Rudwick does in fact put in a qualifying clause excepting the earlier geologists.



Wilson, Lyell's modern biographer, says: 'Lyell sought to separate geology from cosmology and refused to consider the origin of the earth or its state when first formed.' (Dictionary of Scientific Biography, p. 568). Analogously, Darwin and his circle refused later to take any notice of apparently strong refutations of his time scale by Kelvin and Jenkin based on established paradigms of physics.

12. De Beaumont (ref. 2), p. 246.
13. Address, p. 307.
14. Ibid., p. 308.
15. Ibid., p. 312; Clark & Hughes' statement (1, p. 370), that it was the study of de Beaumont which led Sedgwick to reject the idea of a universal flood seems highly unlikely as he was already expressing doubt of it by 1829, and had renounced it in his 1830 Presidential Address in which absence of any reference to de Beaumont makes it probable he was unaware of de Beaumont's theories.
16. Secord, Cambria/Siluria: The Anatomy of a Victorian Geological Debate, p. 83. In a footnote Secord does admit that the name of de Beaumont does not appear in Sedgwick's actual notes 'nor would one expect it to'. He adds that his account of de Beaumont's influence is 'an interpretation based both on the use of strike in the notebooks and on Sedgwick's general acknowledgement of his indebtedness to the theory of parallelism at this time.' (chapter 3 note 48). He calls Sedgwick a 'follower' of de Beaumont on p. 151.
17. Murchison's notebooks for the early 1830's (in the Geological Society) show e.g. on p. 28 for 1832 a tentative section from

the Stiper Stones to the Longmynd having four or five notes each of dip and strike.

18. Secord (ref. 16), p. 44. In fact, of course, all science is the discovery of order and structure, and it is hard to see why geology should appeal more than (say) astronomy or chemistry. Obviously (as Hooykaas and others have illustrated) a belief in a rational Creator goes naturally with a belief that science can discover rationality and order in nature - and it presumably also goes naturally with a view that God has given such order in social or religious affairs. But Secord is surely saying something more than this, concerning the particular form of geological theories.
19. It would, of course, always be open for (say) a Marxist historian to argue that Sedgwick's science was affected by an ideology based on the concrete economic relationships between the Establishment (of which he was a member) and the working classes. It would be very hard to either demonstrate or disprove this, but we would at least be entitled to ask for indications either of a different kind of methodology which he might have applied or a different specific theory he might have espoused, had he adopted an alternative ideology. As I have indicated, Sedgwick's Catastrophism could equally well be associated with a revolutionary creed, and his methodology was consciously based on the ideas of two men who lived in a social order of more than a century earlier. His methodology certainly did affect his geology: in, for example, the kinds of assumptions to be made and evidence to be accepted. I have not been able to discern any analogous specific implications of his social or political views - other than a basic belief in a rational universe. The detailed exploration of those views, as

indicated in the Introduction to this thesis, is not undertaken in the present study and would merit further work.

20. See ref. 4 above.
21. The notes on Sedgwick's own copy in Cambridge are, however, not in much detail.
22. 3rd Edition Syllabus, p. 4.
23. Raised beaches are mentioned on p. 9, and p. 10 refers to the proofs of former action of great demuding currents. Sedgwick does mention the systems of Burnet, Woodward, Hutton, Werner, and Elie de Beaumont (all p. 66) and his failure to mention Lyell is interesting. Lyell's uniformitarianism was, of course, not a distinct system as such but an over strong presumption - but his steady state view is not mentioned either.
24. There was, of course, much contemporary comment, and the controversy continued after the deaths of both Sedgwick and Murchison. In recent times, historians such as Thackray ('The Murchison-Sedgwick Controversy', Jour Geol. Soc. Lond., 1976, 132, pp. 367-372) and Rudwick ('Levels of Disagreement in the Sedgwick-Murchison Controversy', Jour Geol. Soc. Lond., 1976, 132, pp. 373-375) have made comment. The detailed study is that of James A. Secord, (ref. 16).
25. The Sedgwick journals are in the Sedgwick Museum, Cambridge.
26. The Darwin journals for the period of the tour are published in Paul H. Barrett, 'The Sedgwick-Darwin Geologic Tour of North Wales', Proceedings of the American Philosophical Society, 1974, 118, 146-164.
27. Ibid., p. 153.

28. Barrett's material on the actual journal content is useful, but his suggestions regarding the religious and personal relations of the two men in the period are extraordinary if not absurd. From the extreme of the Winstanley-Cannon tradition of seeing Sedgwick as a 'Broad Churchman', Barrett goes to the other extreme of transporting him to twentieth century America and making him a 'religious fundamentalist' (p. 146). He was (says Barrett) 'authoritarian' and 'patronising', and was a 'domineering religiously orientated geologist' (p. 149) whom Darwin with his freethinking background would have found hard to bear. This is highly implausible for a number of reasons. First, Darwin himself spoke with evident pleasure of the trip (as Barrett admits) and later retained an affection for Sedgwick even through their controversy. Secondly, Sedgwick was not given to pious pontificating, and his sense of fun made him a generally sought after companion when his spirits were good. Thirdly, Darwin at this time would have undoubtedly shared in Sedgwick's natural theology and views on a Designer (a point reinforced by recent work by Herbert and others). Murchison, incidentally, admitted in a letter to Sedgwick in 1838 (which is published in 'Letters Concerning the Cambrian-Silurian Controversy' of 1852 by C.R. Craig, Jour. Geol. Soc., 1971, 127, 483-500) that he believed in natural theology and was a theist but not a Christian - though he had evidently enjoyed their joint trips and had not felt pressurised by Sedgwick's beliefs. Barrett also introduces a couple of supposed inconsistencies in Sedgwick's thinking. One Barrett creates by misquoting Sedgwick's words: 'We constantly find (fossils) passing into each other (in a series of formations in contact with each other)'. The

parenthesized word 'fossils' is not in Sedgwick's text and he is speaking of formations not fossils. Neither here nor anywhere else does Sedgwick imply that fossils show a gradual change, and Barrett finds inconsistency between this and Sedgwick's rejection of transmutation only because he misinterprets Sedgwick. Similarly Barrett sees inconsistency in Sedgwick's desire to remove denominational barriers to Cambridge University degrees and his belief that the finger of God is seen in nature - though Barrett does not explain what the inconsistency is supposed to be and to many of us it may appear perfectly consistent.

At base, Barrett finds Sedgwick inconsistent because he misunderstands his methodological and religious position.

29. Barrett (ref. 26), p. 141.

30. Ibid., p. 158.

31. Ibid., p. 151.

32. Clark & Hughes, 1, p. 379.

33. Clark and Hughes, 1, p. 391.

34. I was able to spend a week in company with a geologist tracing a route near Snowdonia which Sedgwick galloped through in an afternoon during his tour. His ability to pick out at such speed the main lines of dip and strike were surely remarkable.

35. Secord (ref. 16), p. 337. Some of my own earliest work for this thesis was on the Sedgwick/Murchison controversy, but the publication of Dr. Secord's thesis in 1981 has covered much of the material, and my own treatment necessarily has contracted. I would not dissent fundamentally from any of his conclusions, though in my view he exaggerates the falsity of Sedgwick's later accounts; Sedgwick generally bent the truth rather than falsified it.

36. Secord claims that Sedgwick later greatly exaggerated both his reluctance to accept this and his reliance on the supposedly irrefragible authority of Murchison. In a letter to Murchison on November 7th 1832 Sedgwick did refer to the system around Meifod plunging under the Berwyns and reemerging 'to the East of Bala Lake' from whence he had traced it down to Dinas Mowddu. But at that time he equated it with the Long Mountain (actually on a higher level). Both men seem later to have accepted that the Meifod and Bala were separated at Murchison's instigation - though most geologists felt that Sedgwick should share the blame.
37. Sedgwick, Proc. Geol. Soc. Lon., June 1843, 4, p. 217; the area so designated is, of course, the area around Meifod.
38. Sedgwick, Quart. Journ Geol. Soc. Lon., December 16th 1846, 3, p. 159.
39. Sedgwick, Quart. Journ. Geol. Soc. Lon., February 25th 1852, 7; the Second Facilus of July 1852 also puts Caradoc in Cambrian but above Bala. In a letter to Murchison of November 20th 1842, however, Sedgwick asserts: 'I do not, however, believe the Bala Limestone to be the same bed with the Caradoc limestone on the east side the Berwyns. For if so, the chain of the Berwyns must overlies your Caradoc... which seemed to me a reductio ad absurdum ...' He says it is possible that the Meifod may be low, but he will still 'stick to my old opinion of 1834 - that the Meifod limestone is ... many thousand feet over the Bala Limestone...' He says, however, that apart from 15 or 20 new species, all are published Lower Silurian in the Bala. Perhaps this seemed more significant to him by the 1843 resume.
40. Secord (ref. 16), p. 304.

41. Elsewhere in the February 1852 article (ref. 39) Sedgwick gives the earlier conclusion that Meifod beds were 'a part of the Cambrian system series and the fossils seemed to sanction this conclusion; for the Meifod fossils and Bala fossils seemed to be almost identical in species.' (p. 149)
42. Sedgwick (ref. 39), pp. 149-150.
43. Clark & Hughes, 1, p. 131.
44. Sedgwick (ref. 38). In view of such words, borne out by Sedgwick's own practice, it is surprising to read in a thesis by B. Hamilton the following: 'Earlier though [pre 1850's] the strength of a biblically based theology in an interpretive role of geological phenomena tended to stress the palaeontological side of geology rather than the unfossiliferous stratigraphic aspects.' (The Development of Hard Rock Geology, p. 58). Ironically, here it is Sedgwick (who was fully committed to an Evangelical faith and to the Bible) who espouses structural aspects, against Murchison (who admitted a lack of Christian faith) who emphasizes fossils.
45. Sedgwick (ref. 39), p. 158.
46. Sedgwick, Phil. Mag., November 1854, 8, p. 361.
47. Ibid., p. 369.
48. The letters between Murchison and Sedgwick, unless otherwise stated, are either in the University of Cambridge Sedgwick collection, or in the Murchison collection in the Geological Society of London Library.
49. Murchison, Proc. Geol. Soc. Lon., January 6th 1847

50. Sedgwick's letter, dated 20th October, has already been quoted above - and it confirms the identity of the fossils.
51. In an article, 'King of Siluria: Roderick Murchison and the Imperial Theme in Nineteenth Century British Geology, Victorian Studies, 25, 1982, 413-445) Secord has emphasized the territorial factors with Murchison in relation to his military past (see also Secord (ref. 16), p. 14-16).
52. This intention was obscured by the editorial alteration of his 1844 map for the Geological Society Proceedings to make Protozoic equal to Lower Silurian.
53. Letters in early 1836 carried Sedgwick's criticisms of Murchison's introduction. On March 27th 1838 Sedgwick wrote apologising for the 'violent language' he had used in objecting to parts of the proofs of the Silurian System, and accepting Murchison's own apologies. On 19th April 1838 Murchison wrote explaining circumstances relating to "B's memoretto" which had angered Sedgwick.
54. Secord seems to imply that Sedgwick had 'failed' in his career in the church, and Murchison had in his career with the military. But there is no evidence that Sedgwick was ever 'ambitious' to rise in the church in this sense - and Secord adopts a scale of values relating to the church which a man with Sedgwick's family background may not have accepted.
55. Clark & Hughes, 2, p. 250.
56. A transcript of a letter dated 24th February 1843 states: 'The enclosed is a part of a very wee map of England about to be published by the Society of Useful Knowledge ...' Murchison asks Sedgwick for advice on igneous rocks and non-fossiliferous slates.
57. See Clark & Hughes, 2, p. 251; the article was 'On the Older



- Palaeozoic rocks of North Wales', 1845, 1, 5 - 22.
58. Clark & Hughes' account is on 2, p. 216; Phillips' account (as the referee in the case) is in letters of 16th February and 20th February 1856, in the Sedgwick papers at Cambridge University.
59. Clark & Hughes, 2, p. 255.
60. Clark & Hughes, 2, p. 262.
61. See e.g. Clark & Hughes, 2, p. 324.
62. Sedgwick states this in a letter dated 6th March 1856.
63. Sedgwick's Introduction to the Cambrian and Palaeozoic Fossils, p. 75 and p. 90.
64. The Sedgwick/Murchison letters are in the collections stated in note 48. Also in the Cambridge collection is a letter to Jukes dated 2nd November 1855 in which Sedgwick wrote: 'Murchison has had my introduction for many weeks, but no notice of it has come from him. If he does not do me justice so far as to withdraw all surmise or implication (direct or indirect) that I misled him - there is an end to our friendship. I knew he was a vain man; and I knew that his own reputation was his darling idol, but I was very loath to think that he was ready to sacrifice historic truth to this miserable worship.'
65. Sedgwick believed that Murchison in his anniversary address of 1843 should have corrected the 'general view' as Sharpe saw it.
66. To Lyell on 28th April 1857 (Clark & Hughes, 2, p. 324).
67. Secord (ref. 16), p. 14.
68. As early as a letter of 23rd December 1834 Sedgwick wrote to Murchison: 'In short you have no good base line and an immense thickness of older transition rocks are below your series.'

## 7.5 Methodological Comments in Non-Geological Contexts

### 7.5.1 Introduction

We have considered comments on scientific methodology which Sedgwick made in the context of his actual geological work, and have compared him on this with other geologists of his generation. There were, however, a number of other, more general contexts in which he made comments on methodology, and several contemporary non-geologists whose approaches to science may be usefully compared with that of Sedgwick. The present chapter, then, will begin with a comparison of Sedgwick and his contemporaries, Whewell and Herschel. It will continue with an expansion of Sedgwick's own comments on general scientific methodology. Then, after a brief comparison with Coleridge, it will look at the further particular areas of scientific education and of evolution and explore Sedgwick's methodological comments in each of them.

### 7.5.2 Herschel, Whewell and Sedgwick

As we have seen, during the 1830's those newly named as 'scientists' became more self-conscious as a group. Knowledge, in these early years, was still regarded as a unified whole. But the British Association for the Advancement of Science had more or less explicit views on what 'science' was, as distinct from other kinds of knowledge. In view of this, it is really no surprise to discover that scientific methodology was itself under discussion amongst those recognised as leaders in science. Two of the most important of these were Herschel and Whewell, and we may now consider briefly their methodologies, and compare them with those of Sedgwick.

John Herschel had published his Preliminary Discourse on Natural Philosophy in 1831. Whewell's interest in methodology, already indicated in his Bridgewater in 1833, showed in the

methodologically orientated History of the Inductive Sciences in 1837, and his Philosophy of the Inductive Sciences (dedicated to Sedgwick) in 1840. The importance of both in general, and to men like Darwin, has been emphasized by modern historians of science.<sup>1</sup> On a personal level Sedgwick was closer to Whewell, but his ideas are closer to those of Herschel. Herschel is ontologically naïve. He says that we experience 'signals' from real objects;

'As the mind exists not in the place of sensible objects, and is not brought into immediate relation with them, we can only regard sensible impressions as signals conveyed from them by an... inexplicable mechanism to our minds, which receives and reviews them, and by habit and association connects them with corresponding qualities or affections in the objects...'<sup>2</sup>

The naïveté of this is fairly obvious in that, since we never experience objects 'directly', it cannot be that it is by habit and association that we associate qualities in them with the qualities of the signals we receive. The analogy Herschel uses (of a telegraphic signal heralding the arrival of a ship) is invalid because whilst we do actually experience the ship and so can 'associate' the signal with the ship, we never actually experience objects directly. Nor is this criticism mere hindsight, for any reading of Kant, even allowing for the controverted nature of 'things in themselves', would have revealed the fallacy.

Methodologically, however, Herschel owed little to Kant; like Sedgwick his main mentor was Bacon, followed by Newton. To Herschel, however, Galileo also shared the honours, and he speaks of:

'...that great eclipse of science which was destined to continue

for nearly eighteen centuries, till Galileo in Italy and Bacon in England, at once dispelled the darkness; the one by his inventions and discoveries; the other by the irresistible force of his arguments and eloquence.'<sup>3</sup>

The tenor of this is highly reminiscent of Sedgwick's syllabus, both in content and turn of phrase. Bacon is supreme in philosophy of science:

'It is to our immortal countryman Bacon that we owe... the development of the idea that the whole of natural philosophy consists entirely of a series of inductive generalisations, commencing with the most circumstantially stated particulars, and carried up to universal laws or axioms, which comprehend in their statements every subordinate degree of generality, from generals to particulars, by which these axioms are traced back into their remotest consequences and all particular propositions deduced from them; as well those by whose immediate consideration we rose to their discovery, as those of which we had no previous knowledge.'<sup>4</sup>

Herschel's more specific methodological comment is also Baconian, speaking of 'prejudice of opinion' and 'prejudice of sense' in the style of Bacon's idols, and suggesting 'ten rules of philosophising' which resemble the ideas later taken up by Mill. Newton is seen as applying the Baconian ideas:

'Whatever department of science he touched, he may be said to have formed afresh. Ascending by a series of close compacted inductive arguments to the highest axioms of dynamical science, he succeeded in applying them to the complete explanation of all the great astronomical phenomena, and many of the minuter more enigmatical ones.'<sup>5</sup>

Herschel sees geology as of great importance (next to astronomy <sup>6</sup>)

and as involving inductive generalisations in the manner of Bacon as he understands him.

Sedgwick cites Herschel in his Discourse<sup>7</sup>, but other references are rare and then usually made to Herschel's scientific work rather than to his ideas of science.<sup>8</sup> There is no evidence that Sedgwick derived anything significant from Herschel which was not common to the heritage of Bacon and Newton which they both shared.

The names of William Whewell and John Herschel have sometimes been linked, and yet in fact Whewell's approach to the philosophy and methodology of science is far more sophisticated. His approach to the history of science was philosophical, and yet he also believed a history of science was necessary in order to develop a proper philosophy for it.<sup>9</sup> To anyone interested in the philosophy of science, Whewell is one of the most interesting writers ever, and it would be tempting here to launch into an attempt at a full summary of his ideas.<sup>10</sup> This temptation, however, will be resisted, for there seems little evidence that Sedgwick (for all the flattering dedication of the Philosophy of the Inductive Sciences to him) shared Whewell's sophistication; thus the merest sketch of his ideas here must suffice.

Whewell began his work on the philosophy of science by making various fundamental antitheses. Amongst these are 'things and thoughts': 'thoughts are something which belongs to ourselves' whilst 'things ... are something different from ourselves and independent of us.'<sup>11</sup> Kant-like, Whewell concludes that 'knowledge implies a combination of Thoughts and Things... Without Thoughts, there could be no connection; without Things, there could be no reality.'<sup>12</sup> But although they are inseparable in the mind in any act of apprehending knowledge, philosophy (he says) requires their conceptual separation. Whewell then speaks of the

difference between deduction and induction: 'In the former, we proceed at each step from general truths to particular applications of them: in the latter, from particular observations to a general truth which includes them.'<sup>13</sup> This sets the background for the next fundamental antithesis: Theories and Facts:

'General experiential Truths, such as we have just spoken of, are called Theories, and the particular observations from which they are collected, and which they include and explain, are called Facts.'<sup>14</sup>

This may sound like a good basis for a distinction between facts and hypotheses such as Sedgwick and the Geological Society might have approved. But he goes on to speak of Ideas and Sensations, in a way which reflects far more the active nature of mental states and acts. Kant-like he sees space and time as Ideas because they are general relations among our sensations, apprehended not merely by senses but by an act of the mind.<sup>15</sup> The mind is active, and not merely a passive recipient of sensations; this (again like Kant) forms a starting point for Whewell's critique of empiricism. In Section 7 Whewell makes clear his debt to the German philosophers, e.g. Goethe and Schiller, and his familiarity with that school of philosophy. In Section 9 he reaches the often quoted words:

'Man is the Interpreter of Nature, and Science is the right Interpretation.'<sup>16</sup>

But to Whewell this 'interpretation' is literal, not figurative, and the import of his aphorism has not always been noted even by those who have cited it. If all scientific observation is interpretation, then how can there be a distinction between objective fact and inductions based upon reasoning? Whewell passes immediately to ask this very question. He concludes:

'...in Theory the Ideas are considered as distinct from the Facts: In Facts, though Ideas may be involved, they are not, in our apprehension, separated from the sensations. In a Fact, the Ideas are applied so readily and familiarly and incorporated with the sensations so entirely, that we do not see them, we see through them.'<sup>17</sup>

Whewell is aware that there is a close interrelation between the concepts through which we view and interpret experiences, and the language (in particular technical scientific language) with which the concepts are expressed. Here, again, familiarity makes us forget the difficulties seen when the terms were first introduced, and scientific terms themselves 'carry with them, in their import, the results of deep and laborious trains of research.'<sup>18</sup>

Those who have emphasized the 'theory-laden' nature of observation<sup>19</sup> have two particular questions to face concerning the nature of 'truth'. The first is whether theories can ever be more than mere instruments for prediction, that is, be 'true' in a more literal sense; the second is whether one can ever be certain that a particular theory is 'true' in whatever sense of that term one arrives at.<sup>20</sup> Whewell denies that there is 'any ground of general scepticism with regard to truth'.<sup>21</sup> He asserts that 'Geometry and Arithmetic' are areas in which we have certain knowledge - though it is not clear as to whether he means areas within these which later he tells us contain necessary truths (i.e. truths the denial of which would be self contradictory). He adds, however, that: 'The doctrines of Astronomy are examples of truths not less certain respecting the Facts of the external world.'<sup>22</sup>

This is not very convincing and Whewell gives no elaboration of what facts are supposed to be certain. Since he has just admitted that the supposed Fact of the stationary earth had at one time the strongest apparent evidence, astronomy is a particularly strange area to choose.

At certain points Whewell parallels Sedgwick. He accepts much of Bacon's criticisms of Greek philosophy.<sup>23</sup> But his analysis is very careful, and distinguishes in detail between different Greek schools and philosophers. There may also seem to be a parallel in the idea of science as a series of inductive and ascending generalisations. Thus Whewell says:

'And thus, beginning with the facts of sense, we gradually climb to the highest forms of human knowledge, and obtain from experience and observation a vast collection of the most wide and elevated truths.'<sup>24</sup>

Criticising the Vestiges, Sedgwick says that the author:

'...Is intensely hypothetical... without having any just conception of the methods by which men, after the toil of many generations, have ascended step by step to the higher elevations of physical knowledge - without any even glimmering conception of what men mean when they tell us of Inductive Science and its sober truths.'<sup>25</sup>

Both men saw science as an ascent to higher generalisation, and both saw it ultimately as based on induction: neither had a strong conception of the hypothetico-deductive model. Yet the apparent similarity may cloak a basic difference in the way that similar language is perceived. To Whewell all observation involves interpretation, and concepts and theories become 'facts' by familiarity. None of Sedgwick's writings make any such point, and this in spite of a great deal of comment on observation and science. Neither Sedgwick nor Whewell were ultra-empiricist, both rejected the extension of Lockean empiricism into the moral field. Both also rejected the utility approach to morals which correlated with empiricists such as J.S. Mill in their era. Yet, at base, Sedgwick stood much closer to the true Baconian inductivist-empiricist tradition than Whewell. Whilst both Sedgwick and Herschel saw the 'ascent'



of knowledge in Baconian terms, to Whewell it was very much mediated by Kantian thinking. Sedgwick actually refers very little in his writings to Kant. He refers to his idealism<sup>26</sup>, and says that Kant is not to blame for his followers.<sup>27</sup> He also refers to a paper by Whewell on Kant<sup>28</sup>. He gives a very Kantian reply to Locke - though without mentioning Kant's name.<sup>29</sup> But, and this in spite of Whewell's flattering dedication of his Philosophy of the Inductive Sciences to Sedgwick, he is not saturated with Kantian thinking in the way that Whewell is.

There are certain paradoxes about Whewell. His reputation during his lifetime was immense, and extended to all factions in the church as well as to the scientific community.<sup>30</sup> Yet even contemporaries like J.S. Mill failed to understand his position<sup>31</sup>. After his death the lack of understanding seems to have continued with few exceptions, and his works fell into a strange obscurity whilst the less sophisticated and original works on science by J.S. Mill continued to be better known.<sup>32</sup> With the apparent reinvention of some of his ideas in the twentieth century<sup>33</sup>, perhaps the scene was set for the partial renaissance in appreciation of Whewell which has come since the 1960's.

Though surprising in itself, the apparent lack of understanding of Whewell's philosophy, and its obscurity after his death, at least reduces our surprise that the writings of Sedgwick (who is so often linked with Whewell) not only make few references to Whewell, but contain little or nothing distinctively 'Whewellian' in philosophy.

### 7.5.3 Sedgwick, Generalisation and Theory

What I have called the 'later' period of Sedgwick dates from around 1831. In 1833 Sedgwick published the first edition of his A Discourse on the Studies of the University. This, as one might expect, contained some comment on the nature and place of science.

Bacon, Newton, Ray and Barrow are all eulogised, with a particular accent on Newton.<sup>34</sup> The study of the laws of nature, says Sedgwick, is arduous:

'Before he can reach that elevation from whence he may look down upon and comprehend the mysteries of the natural world, his way is steep and toilsome, and he must read the records of creation in a strange, and to many minds, a repulsive language, which rejecting both the senses and the imagination, speaks only to the understanding. But when this language is once learnt, it becomes a mighty instrument of thought, teaching us to link together the phenomena of past and future times.'<sup>35</sup>

Sedgwick was not a naive inductivist.

Sedgwick's appendices also contain comment on science. Picturing Newton's work on celestial mechanics as a series of assumptions and deductions Sedgwick concludes:

'Nothing can be conceived more perfect than this induction; which, starting with laws ascertained by observation, ascended by successive demonstrations, and proved that the most striking phenomena of the solar system were necessary truths involved in the operation of one single mechanical law.'<sup>36</sup>

Sedgwick goes on to quote at length Newton's words on hypotheses and the method of analysis.<sup>37</sup> Sedgwick applies Herschel's words 'induction' and 'deduction', and states that in the method of induction and analysis Newton is unrivalled.

For deduction Sedgwick sees two roles:

- (i) 'in deducing from first principles truths already known by observation'
- (ii) 'in deducing consequences hitherto concealed in the unexplored regions of nature.'

He finishes the section by asserting that, of course, such methods only apply to physical phenomena, not to moral or psychological ones. But in any context there is no concept of the hypothetico-deductive method. Deduction only operates on truths already known from induction. The inductive process led Newton to 'necessary' truths about the operation of nature. In other words, Newton continues to be seen as a supreme exemplification of Baconian induction.

Sedgwick's first edition of the Discourse was in 1833, two more editions followed in 1834, and a fourth in 1835. None added anything significant on the nature of scientific methodology. The 1840's, however, saw Sedgwick entering further controversy in which he commented on the nature of scientific method. In 1844 Sedgwick replied to a paper by William Cockburn, Dean of York, in the British Association. As reported in The Athenaeum, Sedgwick said:

'Wherever truth can be expressed in language, it is done as a generalization. As we advance in the discoveries of science, facts multiply so fast upon us that they would become unmanageable, if we could not group them by certain resemblances, or include them under some more simple law, which is merely an expression of a general conclusion derived from facts which we know to be true, and from which all those phenomena proceed as necessary and inevitable consequences. The moment we arrive at the knowledge of such a law, we can assume, in a sense, a prophetic character, and predict events with certainty, because we know that the Author of Nature is unchanging in his operations, and that the same effects will follow the same causes in times to come as in times past.'<sup>38</sup>

Sedgwick has here, of course, an answer to the Humean problem of

the 'law of uniformity'. Unlike, say, J.S. Mill, who sought to support such a law through a process of a widebased inductive argument, Sedgwick sees it as an implication of the nature of a consistent God. To those of the religious persuasion of Hume or Mill such an option was not available.<sup>39</sup>

Within his own intellectual framework, Sedgwick's belief in the law of uniformity is sound. But what is (by modern standards) inadequate, is his picture of the way in which generalisations are made. Facts are 'grouped together by certain resemblances', and a general law is 'derived from facts which we know to be true'. Neither elements of the hypothetico-deductive approach, nor a concept of paradigm-change, are present here. Sedgwick goes on, however:

'We meet together here to extend our generalizations by new facts, or to modify those laws at which we had previously arrived by embodying all the new truths we have attained, so as to bring our generalizations up to the condition of present knowledge. In some cases we have tested our general conclusions so often that we are as certain of their truth as we are of our own existence. There are others in which we have not arrived at any such certainty, and it is exactly such conclusions, and the facts connected with them, that we meet here to discuss. Even in astronomy there are still certain residual phenomena, at present not fully explained; but in a new science like geology ... it is most advantageous that collections of facts, brought here by observers with different views, should be closely examined, in order that one may check another, and that laws of phenomena be made out, before anyone presumes to put forth any theory of the earth and its formations.'

Sedgwick here speaks of 'modifying' laws, and of 'testing' them. However much in theory a scientist of Sedgwick's calibre might believe in Baconian induction, he must in practice be aware that the process is fallible. Yet his words may still be read (in view of his attitudes to earlier 'mistakes' in his geology) as seeing such modifications as being needed not because observation involves interpretation and a better interpretation might be found, but because mistakes are made in imperfectly applying the inductive process.

A further aspect is indicated in the last sentence of the quotation. Sedgwick sees the process as climbing to ever wider levels of generalization. Theories of the earth's formation are at the widest possible level, and can come only after the lower order laws are known. He goes on:

'...the discussion of broad theoretical questions and cosmogonies, like those now brought before us, is utterly unfit for the present meeting.'

He is especially concerned that anyone might (say) try to overturn the whole basis of established geology without 'any personal knowledge of the subject, or a single new fact to offer'. The BAAS purpose is 'the examination of facts, either to modify our theories and generalizations if we have gone too far, or to bring into harmonious order our new facts, by some new and noble generalization.' Again we note the phrase if we have gone too far - if the inductive process has been imperfectly applied. Although the change from (say) Wernerianism to later geology, or Diluvialism to his later views, were both clear paradigm changes, both would be seen by him as necessary because he had 'gone too far' before. Moreover, to Sedgwick, (although he was not in fact totally opposed to theory) facts and theory were sharply defined:

'Everyone who brings a statement of facts to this meeting, asserts his willingness to abide the test of observation and experiment; and when a paper is brought here which deals not with facts, but with theories and cosmogonies, we should reject it altogether, as in its nature unfit for our notice... At the same time we are willing to show, on all proper occasions (though this be a very improper one), that we are not afraid of facing any of the difficulties with which the speculative part of our subject may be surrounded.'

This is, of course, totally at variance with Whewell's philosophy (published four years earlier) in which facts are seen as familiar theories. The context in which Sedgwick sees this (as the rest of the article shows) is one of geology. The facts of the types and levels of strata are there -actually Cockburn was clear about accepting them as the geologists claimed them to be. Now Cockburn was putting forward an alternative theory to explain them. Sedgwick goes on to totally demolish Cockburn's theory, showing that it simply cannot account for the phenomena known. But why does he object to the presentation of such a theory per se ? What is the difference between a Bucklandian general theory and a Cockburnian one ? Sedgwick's answer to this is not clear, for he was struggling to deal with methodological elements which his Baconianism did not really equip him to deal with. Suppose we grant that there are 'facts', is there a difference between Baconian inductive generalisation and making theories or speculations ? In any event, are such speculations in general improper for the BAAS, or just those which are ill informed ? Later Sedgwick says:

'In determining the succession of the strata, or any other problem in our science, we must be content to ascend, step by

step, from small assemblages of facts, to higher generalisations, until we obtain the whole sequence.'

Is this a ruling out of speculation per se, or is it just ruling out speculation which is made in ignorance of particular 'facts' already known? His actual criticisms of Cockburn turn around the point that Cockburn's theory itself contradicts known facts - unlike mainstream geology his theories have not 'called in the aid' of other specialists such as zoologists, botanists and chemists in order to 'regulate our conclusions by their evidence'. Yet Sedgwick's own earlier comments seemed to be objecting to speculation or theory of any kind, not just ill informed ones. Finally, I regard this question as unresolvable, because the evidence seems to be that Sedgwick himself was not sure exactly where his objection lay. He had what we might call a 'gut reaction' against certain types of theory, but his Baconianism did not provide him an adequate framework to formalise his objections into a systematic and consistent position.

Similar comment might be made about Sedgwick's reactions to the Vestiges of the Natural History of Creation (published 1845). We have already seen how, in both published and unpublished material, Sedgwick reacted violently both to its materialism and to its unscientific nature. In a letter giving virtually his first reaction on 10th April 1845 Sedgwick spoke of its 'utter ignorance of what is meant by induction.'<sup>40</sup> In the actual review he wrote:

'The author is intensely hypothetical, and builds his castles in the air, misconceiving the principles of science, or misunderstanding the facts with which it has to deal; or, what is worse still, distorting them to his purpose. He does all this, apparently, without having any just conception of the methods by which men, after the toil of many generations, have ascended step by step, to the higher elevations of

physical knowledge - without any even glimmering conception of what men mean when they tell us of Inductive Science and its sober truths.'<sup>41</sup>

'... the ascent up the hill of science is rugged and thorny... and ways must be passed over which are toilsome to the body, and sometimes loathsome to the senses. And every one who has ventured on these ways, has learned a lesson of humility from his own repeated failures. He has learned to appreciate the enormous and continued labour by which every new position has been won;...'<sup>42</sup>

Chambers is also misled by superficial resemblances, is hypothetical, and is credulous. Sedgwick adds:

'A hypothetical spirit is a good spirit, if it be properly tempered with knowledge, honesty and sagacity. It is but a perpetual upward tendency, and a craving for some higher principle, to bind together new phenomena and disconnected facts. When thus tempered it leads us not to worship our first imaginations, and to make all nature bend to them, but it makes them bend to nature... This has been the governing principle of the two Herschels, father and son, of Black, of Davy, of Dalton, and other great names in modern discovery.'<sup>43</sup>

The erudition of Sedgwick sometimes may mislead us into thinking that he has actually been more specific than he really has. His words on hypothetical spirits could be read as reflecting a Whewellian position - but it really does not. He has no idea of hypotheses becoming 'facts' by familiarity. Rather, he may be thinking more of what we might call 'conjectures', which we then seek to check and verify. He describes in detail his views of the nebular hypothesis, concluding:



'It is a splendid vision, and may vanish in mid air; or, after five hundred years of continued observations, it may pass into a good substantial theory.' At present, it is utterly unfit to form the basis of any system of nature, such as our author presumes to erect on it...'<sup>44</sup>

The nebular hypothesis, as put forward by the Herschels, was a conjecture, but Sedgwick thought observation would decide it. We may also note that he is not hesitant in inferring geological agents. 'Igneous matter has during many periods been protruded from below - that mountains have risen in succession from the sea.'<sup>45</sup> Gigantic boulders 'have been driven by floods across our continents, or drifted by icebergs over our valleys, and perched sometimes on our mountain tops... There were enormous changes of level; and glaciers as well as floods played their part in producing these strange phenomena...'<sup>46</sup>

Perhaps at base Sedgwick objects again (as with Cockburn) to the attempts to erect grand hypotheses by someone who is woefully ignorant of the basic accepted 'facts' which that hypothesis seeks to explain. This is his point both about the arduous road to knowledge and on bending theory to fact and not vice versa. He does not object to conjecture or hypotheses if they lead to further observation and are made in accord with known relevant facts.

In 1850 Sedgwick published the fifth edition of his Discourse on the Studies of the University of Cambridge. It contained the text of the original Discourse, sandwiched between a gigantic preface and a huge set of appendices. Both of these show a preoccupation with the issues of the Vestiges, seen by Sedgwick in a context of much wider issues. Both ramble seemingly haphazardly through numerous tenuously connected topics, with much repetition and overlap. Yet, during the course of the work, Sedgwick does make a

number of statements about the nature of science.

As with earlier Sedgwick works, Bacon is seen as the maestro of thinking on the nature of scientific induction, and is often quoted.<sup>47</sup> As before, Sedgwick's inductivism gives him no clear guide on what attitude to take to theory or hypotheses. He asserts:

'All natural knowledge is based on inductive reasoning. We have learnt to comprehend the mechanical movement of the heavens by first learning the laws of motion upon the earth. In like manner we have learnt to speculate securely on the functions of organised being, during the old conditions of the earth, by first studying the laws of organic life among the phenomena of living nature. In every instance we must begin with what is known and present to us, before we can speculate about what is unknown and remote. To this rule we know of no exception.'<sup>48</sup>

Sedgwick does not use the word 'speculate' here pejoratively. Shortly after he adds:

'A good theory embodies in verbal propositions our conceptions of natural laws; and these conceptions are all based on observation, experiment, or good analogy... A hypothetical spirit may do good service; provided it urge us on to make new experiments; but if we rest content with it, and, above all if it lead us, as it has too often done, to shut our eyes against facts, and to take from nature no response but such as suits our fanatical belief of what nature ought to be, it must do deadly mischief to the cause of inductive truth.'<sup>49</sup>

It is not clear in what sense a theory is 'based on' observation etc. But in any event Sedgwick sees the role of hypotheses as heuristic not as an end product. His idea of progress comes out again later:

'In an advancing science, our theory may be true or false, perfect

or imperfect; but as it professes to start from ascertained phenomena, so must it continue to be in co-ordination with such facts as come before us during our progress, or it is good for nothing.'<sup>50</sup>

By the 1850's, one can detect a certain tension between Sedgwick's underlying Baconianism, and his recognition as an intelligent scientist (perhaps with some effect from Whewell ?) that speculation is an important part of creative science. On the one hand he wants to say that theory simply 'embodies' facts, but on the other he recognises that theory can be both speculative and yet heuristically useful even if later modified. This tension is reflected in his later words:

'A theory must be based on well-observed facts, or it is worse than nothing. For theory is but the embodying of facts and phenomena under the form of general propositions, drawn out in accordance with the laws of our intellectual nature: and as our knowledge advances, whether we be led to extend or limit the meaning of our first propositions, by the test of observation or experiment ~~must~~ they stand or fall.'<sup>51</sup>

A second tension exists between again his acceptance of the importance of speculation, but his deep-seated Baconian fear that it will harden into unscientific dogma. Thus he says:

'...the best experimenters have ever been the most successful speculators. Theory is not the idol, but the animating soul of advancing knowledge; and is then only mischievous when it is set up that it may be worshipped, as if it contained the highest truth; though it be built on an untried foundation, and raised only to the level of our shallow knowledge...'<sup>52</sup>

Associated with good speculation are imagination and the use of analogy:

'Kepler, by some strange notions of celestial harmonies, was led onwards to very great discoveries.... The wildest physical notions have sometimes led to truth: and we may, perhaps allow that a theoretical notion springing up in the mind of man must have some suggestion from nature - either as she dwells within himself, or communicates with him through his senses. No one can tell where analogies are to cease, such as Nature's riches. Let us, then, not despise mere analogies; but let us not worship them. Let us test them honestly; and never pretend to build upon them till they are brought into accordance with material facts, and under the domain of material law.'<sup>53</sup>

According to Sedgwick, Kepler may be contrasted with the Ideal School in that the former was prepared to follow inductively ascertained facts, whilst the latter 'conjure up analogies... affirm them without proof, ... and build upon them as if they were the accepted principles of positive material science.' Speaking of the Idealists (as we may see later) Sedgwick is on reasonably sure ground. But one cannot help but feel that some of his applications of praise or stricture to the use of speculation and theory have been much informed by hindsight. One precondition is clear: 'A theory is worse than nothing if it reflect not back the present condition of our knowledge.'<sup>54</sup> But his strictures on past geology go beyond this as he says that geologists' 'besetting sin has too often been a rash and intemperate spirit of speculation.'<sup>55</sup> Later he is more specific:

'Of all material sciences, Geology offers perhaps the wildest field for speculation; yet, in its true history and progress, no science is more practical and inductive. I believe that the premature theories of Werner's school retarded its progress by

more than twenty years.'<sup>56</sup>

Yet he does not tell us how at that time they could have been recognised as premature. His picture of contemporary geologists is suitably rosy for presentation to the world:

'They are not anxious to form any theory: and if as a matter of speculation ~~they~~ do construct a theory, they profess to base it on allowed facts and not on vague assumptions, which the progress of knowledge may prove to be untrue; and at every moment of their progress they are ready either to modify it, or to abandon it altogether, as new phenomena rise up before them.'<sup>57</sup>

At times, indeed, he seems to see in this light even things he appears to assert dogmatically. The author of the Vestiges and Explanations has argued that linking species may be found to substantiate the theory of evolution, but Sedgwick asserts that (as they stand) the organic phenomena of geology do not suggest transmutations.

'And it is no reply to tell us in return, that other facts may hereafter be brought to light so as to modify the nature of the evidence before us. This is the inevitable condition of every advancing science, and is an excellent reason for using much caution in the propagation of any positive dogmatic theory.'<sup>58</sup>

This is ambiguous. It could be read as stating that all scientific law is tentative. Yet, elsewhere, Sedgwick makes statements like:

'in [Newton's] hands Kepler's laws ceased to be empirical laws, and passed into the highest grade of deductive laws. They became necessary truths involved in the very highest and most exact conception of a physical truth that man has ever reached.'<sup>59</sup>

Kepler's laws were, in a sense, 'deductive' given Newton's basic assumptions about gravity and given a set of initial conditions. But in what sense Newton's assumptions were necessary truths Sedgwick

nowhere clarifies.

He does speak of how observation may lead to inductively derived abstract generalisation, which in turn forms a base for deduction:

'Facts, at first apprehended by sense, may in the end pass, by successive acts of the mind guided by successive experiments, to the side of abstract or ideal truth, comprehended under our conception of a physical law. In this new condition they become the root of deductive truths - not now in the way of experiment, but by the efforts of pure reason.'<sup>60</sup>

'We confound not the discoveries of pure reason with the discoveries brought out by experiment; but we affirm that in all physical reasoning the two are bound together, and that the former cannot have any existence without the latter.'<sup>61</sup>

Laplace is there used as an example, and in a later place Sedgwick cites also the discoveries of Airy on modifications to Newton's rings, and of Adams and LeVerrier of Neptune.<sup>62</sup> But Sedgwick is at pains to emphasize that this use of deductive reasoning begins from inductively established laws - the observation and induction come first. He has no time at all for pure rationalism.

#### 7.5.4 The Methodologies of Sedgwick and Coleridge Compared

Methodology is one final context for consideration of the hypothesised 'Coleridge' connection. Coleridge was very interested in science, and had close connections with Davy. Like Sedgwick, he praised Francis Bacon, but his perspective of Bacon differed radically from that of Sedgwick. To Sedgwick, as we have seen, Bacon heralded a new age of enlightenment, far removed from the Greek rush to generalise which had led to 'idols'. To Coleridge, Bacon was the 'British Plato', and his view of Bacon was distinctly rationalistic. Levere has explained this:

'Francis Bacon had stressed that the "forethoughtful theory was "the prior half of the knowledge sought," and insisted that experiments, planned in advance, were decisive. Coleridge made much of this aspect of Bacon, describing him as the inventor of the view of "Experiment, as an organ of reason." With Bacon, Coleridge announced, "as with us, an idea is an experiment proposed, and experiment is an idea realised."<sup>63</sup>

Coleridge could therefore be contemptuous of the Hookean programme for 'fact gathering', and emphasize the active participation of the mind in an emphatic Kantian manner. We have already seen how it is possible to 'read into Bacon' all kinds of things, and it is most doubtful that Bacon would have recognised Coleridge's version of his system. Certainly he emphasized experimentation as the interrogation of nature, rather than passively observing what nature might chance to show us, but this is not the same as recognising the role of imagination in concept formation and the theory-laden nature of concepts. Even the experimentation issue forms little common ground in a comparison with Sedgwick. In chemistry, which was Coleridge's own particular interest, the use of experiments is paramount, but in early nineteenth century geology the word has to be used in a special sense to make it important. Thus Sedgwick and Coleridge took different aspects of Bacon to emphasize.

On Newton they again differed radically. Levere explains:

'Kant and Laplace had built their evolutionary cosmologies on the foundation of Newton's physics. Coleridge, however, saw the laws of the planetary system as Kepler's discoveries, . . . believing that Kepler was a far greater genius than Newton.

This was a viewpoint widely shared by the Naturphilosophen in Germany, who admired Kepler's dynamism, his search for unity and harmony, his recognition of the active role of the mind in natural science, and even his efforts to deduce a priori the ideal geometric structure of the solar system.<sup>64</sup>

Coleridge saw Newton as tainted with Lockean materialistic empiricism, and might be described as 'damning him with faint praise'. On this, as also on the respective systems of Kant and Laplace on the solar system, Coleridge felt that the imagination or idea of the system was paramount, the mathematics was clever stuff but mostly routine. This is in sharp contrast to Sedgwick, who respected Kepler and saw him as a precursor of Newton, but rejected any Hegelian or Naturphilosophie interpretation of his work.<sup>65</sup> In truth, the whole basic sympathies of Coleridge and of Sedgwick were different. Levere states:

'Coleridge's Theory of Life drew on Oken, Steffens and Schelling, who in turn were indebted to Kant.'<sup>66</sup>

Sedgwick mentions Schelling in a context of general disapproval of the school<sup>67</sup>, and a criticism specifically of Coleridge for being influenced by it.<sup>68</sup> Oken is mentioned a great deal, and in great detail by Sedgwick<sup>69</sup>. But in Sedgwick's view, although some of Oken's actual biology was useful, a study of Oken's work reveals:

'... the depths of mysticism, pantheistic profanity, and arrant nonsense, into which a very clever, inventive, and well informed physiologist may sink, when he deserts the track so nobly delineated by Bacon, and so gloriously trodden by men like Galileo, Newton, La Place and Cuvier. By casting away the



lessons of inductive truth from the ample stores of his mind, and taking on himself to spin a web of creation out of his own brain, by a priori reasoning, he has not illustrated the ways of truth - he has not honoured nature, but most shamefully disfigured and distorted her...<sup>70</sup>

Coleridge may have had his criticisms of Oken, but one can hardly imagine him using such language as this. The differences between the two men, on this as on most other issues, is profound.

Though Sedgwick at times gets near to a very dynamic view of the role of speculation or theory, he always felt in his heart (in a way in which, say, Whewell did not) that it was sharply defined from inductive law. Thus in the most full version of his views on the Nebular Hypothesis Sedgwick states:

'It is not brought before us as a sure induction from fact or observation; but rather belongs to a class of speculations, which, whether true or false, go beyond our material knowledge...

It is at present nothing better than a splendid vision; and before it can be received as a physical reality, it must be supported by better evidence...<sup>71</sup>

Sedgwick seems to distinguish three types of entity. In an appendix dealing with 'Laws', Sedgwick states:

'When we tell of the union of dead gelatinous matter with the galvanic fluid as the beginning of organic life, and call this a law of nature, we violate the integrity of language. We are not describing a law made out inductively, but we are starting an hypothesis.<sup>72</sup>

A hypothesis may become an empirical law if evidence later supports it,

'... to be a good hypothetical law, it ought to be suggested by the accepted facts of nature, and tested by them; so that it may pass into the form of an empirical law, or be rejected altogether.<sup>73</sup>

An empirical law 'a mere law of observation', admits of degrees: 'but in its best form it irresistably compels our belief.' But Sedgwick seems to conceive a yet higher form of law. He states:

'A material law may be a demonstrative consequence of an accepted law about which no one doubts; in which case it is a representative of the very highest form or material truth, and admits not of degrees.'

Sedgwick applies this to Kepler and Newton. In Newton's hands:

'Kepler's laws ceased to be empirical laws, and passed into the highest grade of deductive laws. They became necessary truths...'<sup>74</sup>

It is a mystery how so strict an inductivist as Sedgwick could somehow derive a more certain and necessary truth than 'an empirical law', with only an empirical law upon which to base itself.

#### 7.5.5 Sedgwick, Methodology and Examinations

Around the time of the publication of the 5th Edition Discourse, changes were taking place in Cambridge education. In 1845 Whewell had published Of a Liberal Education. Amongst its various comments, the book suggested the possibility of an alternative Tripos for the 'progressive sciences' - amongst which Whewell included geology.<sup>75</sup> Both Oxford and Cambridge at this time could be said to be failing to provide any real incentive to the study of the sciences. Attendance at Sedgwick's lectures was purely for interest, it led to no qualification - and Whewell was suggesting that henceforth Professors would examine. In 1848 Whewell's suggestions bore fruit, and Natural Sciences Tripos were established. Sedgwick supported this introduction. Though:

'Our highest prizes will still be carried off by those who reap their honours in the Mathematical and Classical fields... we are bound to produce good intellectual food for all our sons;

and if they make good progress, even in what we regard our more humble and less exact studies, to give them a commensurate reward and a public honour.'<sup>76</sup>

This was how Sedgwick saw the Natural Science Tripos. Mathematics was still the queen, but other sciences might take a lesser place. This attitude is well in accord with the general attitude at Cambridge in this period, as D.B.Wilson has recently commented.<sup>77</sup> Roy Porter, in an interesting paper, has examined the effect on Cambridge geology of the coming of the Natural Sciences Tripos.<sup>78</sup> As well as commenting on Sedgwick's discursive but highly stimulating style of lecturing, Porter gives some comment on Sedgwick's style of examinations. He refers to the unique 'breadth, freshness and intellectual vigour' in Sedgwick's early examination papers. The 1855 paper has a note appended:

'If in any instance you think you differ in opinion from the proposer of the above questions, you are requested to state your own opinions without hesitation and to give your reasons for them.'

Porter comments on the flow of Sedgwick's papers, working from cosmology through physical geography into stratigraphy and palaeontology, and usually climaxing with a question on evolution or creation.<sup>79</sup> This was a test of the general grasp of natural science and the place of geology in it - rather than a technical test of details of geological strata or fauna. The papers, as Porter remarks, show an interest in methodological patterns of thought, the evaluation and testing of alternative theories, and the proper methods of induction. In short, the paper concerns itself with methodology more than detail. Some of us, indeed, may regret the gradual demise of methodological and philosophical elements in scientific examinations which later occurred.<sup>80</sup>

But, rightly or wrongly, Sedgwick did not see himself as educating technicians - but having the twofold aim of giving a liberal education and at the same time opening a perspective on scientific discovery and inductive research methodology.

#### 7.5.6 Sedgwick, Evolution and Scientific Methodology

The final topic around which Sedgwick made interesting methodological comment concerns Darwinian evolution. On a personal level, Darwin was introduced to Sedgwick by Henslow in 1831<sup>81</sup> and later that year joined Sedgwick for a tour of Wales. Darwin always afterwards spoke warmly of this, and later told one particular story of an occasion when he was disposed to believe a local labourer that a tropical shell had been found in a gravel pit. He continues:

'I told Sedgwick of the fact, and he at once said (no doubt truly) that it must have been thrown away by some one into the pit; but then added, if really embedded there it would be the greatest misfortune to geology, as it would overthrow all that we know about the superficial deposits of the Midland Counties... I was then utterly astonished at Sedgwick not being delighted at so wonderful a fact as a tropical shell being found... Nothing before had ever made me thoroughly realise, though I had read various scientific books, that science consists in grouping facts so that general laws or conclusions may be drawn from them.'<sup>82</sup>

Whilst Darwin was on the Beagle, Sedgwick, along with Henslow, was one of the main instruments in making his name known.<sup>83</sup> Darwin relates in his autobiography that towards the close of the voyage his sister wrote that Sedgwick had called on Darwin's father 'and said that I should take a place among the leading scientific men.'<sup>84</sup> The later controversy did not destroy their friendship, and in 1870 Darwin recorded that Sedgwick was 'most cordial and kind' on his visit

to Cambridge.<sup>85</sup>

Some personal feelings, however, did appear. In Darwin's Origin of Species the following passage appeared:

'Although I am fully convinced of the truth of the views given in this volume under the form of an abstract, I by no means expect to convince experienced naturalists whose minds are stocked with a multitude of facts all viewed, during a long course of years, from a point of view directly opposite to mine. It is so easy to hide our ignorance under such expressions as the 'plan of creation,' "unity of design," etc., and to think that we give an explanation when we only restate a fact. Anyone whose disposition leads him to attach more weight to unexplained difficulties than to the explanation of a certain number of facts will certainly reject the theory. A few naturalists, endowed with much flexibility of mind, and who have already begun to doubt the immutability of species, may be influenced by this volume; but I look with confidence to the future, - to young and rising naturalists, who will be able to view both sides of the question with impartiality. Whoever is led to believe that species are mutable will do good service by conscientiously expressing his conviction; for thus only can the load of prejudice by which the subject is overwhelmed be removed.'<sup>86</sup>

From a philosophical point of view this passage could be seen as extraordinarily prefiguring the Kuhnian concepts of what is involved in a revolutionary paradigm change. But, on the other hand, to an 'inductivist' like Sedgwick, it was not very flattering. The implication was that men like Sedgwick were partial and had inflexible minds. Though modern critics may have accused Sedgwick of this<sup>87</sup>, it is interesting to note that in 1868 (at the age of 83) Sedgwick changed his mind on

the antiquity of man.<sup>88</sup> Whatever the truth or otherwise of Darwin's statement, however, Sedgwick did not like it. Writing to thank Darwin for sending him a copy of the book, Sedgwick commented:

'Lastly, then, I greatly dislike the concluding chapter - not as a summary, for in that light it appears good - but I dislike it from the tone of triumphant confidence in which you appeal to the rising generation (in a tone I condemned in the author of the Vestiges) and prophesy of things not yet in the womb of time, nor (if we are to trust the accumulated experience of human sense and the inferences of its logic) ever likely to be found anywhere but in the fertile womb of man's imagination.'<sup>89</sup>

Sedgwick objected not merely to Darwin prophesying (for after all he goes on to make his own prophecy), but to the triumphalism associated with it. Darwin tried to explain away his words thus:

'My mention of young men being best judges of new theories [?] was not intended for scientists, for however erroneous I remember nearly twenty years ago laughing with Lyell over the idea...'<sup>90</sup>

This reply is totally unconvincing, as even a cursory glance at the original passage will reveal. But Darwin seems to have had a genuine affection for Sedgwick - even in letters to third parties in the height of controversy he refers to him as 'dear old Sedgwick'<sup>91</sup> - and may have felt some qualms at having written a passage which few scientists in Sedgwick's position would have failed to find insulting.

Sedgwick's letter to Darwin states his methodological objections to the book:

'You have deserted - after a start in that tram-road of all solid physical truth - the true method of induction, and started us in machinery as wild, I think, as Bishop Wilkins's locomotive that was to sail with us to the moon. Many of your wide conclusions are based upon assumptions which can neither be proved nor

disproved, why then express them in the language and arrangement of philosophical induction ?'

This is a key accusation made by Sedgwick. He repeats it in private letters, e.g. to Owen:

'Darwin has deserted utterly the inductive track - the narrow but sure track of physical truth,- and taken the broad way of hypothesis, which has led him (spite of his great knowledge) into great delusion, and made him the advocate instead of the historian...' <sup>92</sup>

Around the same time, both versions of his review in the Spectator carried the following:

'Darwin's theory is not inductive, - not based on a series of acknowledged facts pointing to a general conclusion, - not a proposition evolved out of the facts, logically, and of course including them...' <sup>93</sup>

Darwin made no direct response to this point in his reply to Sedgwick, but he did refer both to Sedgwick's letter and his review in a letter to Henslow - who was, of course, a close friend of both men:

'In a letter to me, & in the above notice, he talks much about my departing [about del] from the spirit of inductive philosophy.-- I wish, if you ever talk on subject to him, you would ask him whether it was not allowable (& a great step) to invent the undulatory theory [of light added] - i.e. hypothetical undulations, in a hypothetical substance, the ether. And if this be so, why may I not invent hypothesis of natural selection (which from analogy of domestic productions, & from what we know of the struggle of existence & of the variability of organic beings, is in some very slight degree, in itself probable) & try whether this hypothesis of natural selection does not explain (as I think it

does) a large number of facts in geographical distribution - geological succession - classification - Morphology, embryology etc etc. - I sh<sup>d</sup> really much like to know why such an hypothesis as the undulations of the ether may be invented, & why I may not invent (not that I did invent it, for I was led to it by studying domestic variaties) any hypothesis such as natural selection... I can perfectly understand Sedgwick or anyone saying that nat. selection does not explain large classes of facts; but that is very different from saying that I depart from right principles of scientific investigation.-,94

A number of suggestions have been made as to the real basis of the rejection of evolution by men like Sedgwick. Hull, for example, criticises Alvar Ellegard for viewing the controversy as a 'conflict between empiricist and idealist philosophies of science.',95 Yet Hull's own unsympathetic approach to Sedgwick leads him to the conclusion:

'As is often the case when proper scientific method is invoked, Sedgwick was highly inconsistent in its application. Darwin's reasoning was just a string of bubbles, but his own alternative explanation in terms of creative powers which he could not comprehend was "a legitimate conclusion of sound reason drawn from the laws and harmonies of nature"... the man was incapable of disinterested inquiry.',96

So in what way did the undulatory theories of light, and Sedgwick's own creation theory, differ from Darwinian evolution ? Whilst, with hindsight, we may all accept that broadly speaking Darwin was right and Sedgwick wrong on the scientific issue, is it also true to say that Sedgwick was being inconsistent to his own principles of science ?



Few people in history, and even fewer who cared passionately about anything, might plausibly lay claim to total consistency at all times. Nevertheless there is an underlying consistency in Sedgwick's attitude, given his own views concerning induction and hypotheses. These views were presented above, with quotations, in part 7.5.3, and we might summarize them thus:

- (a) An inductively known 'theory' is an embodiment of, or generalization from, known facts.
- (b) Hypothesis or Speculation may be a useful stimulus to further experiment and observation - it is an 'animating soul'.
- (c) But such hypothesis should 'reflect the present state of knowledge, and at the least not contradict known facts'
- (d) Until inductively demonstrated, such hypothesis must never be presented as though it were the known and certain product of induction.

What Sedgwick was particularly adverse to was the rationalistic approach (of Hegel and the German school), of a priori reasoning to decide what 'must be' the case. To Sedgwick it was both unscientific (for science was based on observation and induction) and impious (in saying what God 'must have' done in creation).

How, then, did he see Darwin's theory? He begins by saying that 'Darwin's theory is not inductive'. Surely few could doubt that, in any meaning of that term which would make sense to Sedgwick, this is simple fact? If Darwin could have pointed to a number of actual observed cases of evolution, and from these generalized, then his theory would have been inductive. But both Darwin and Sedgwick knew that he could do no such thing. We might contrast this, for example, with the hypothetical explanation of a rock formation as a 'raised beach'. In such a case, we see similar structures, now being created by shorelines, generalize to an inductive law, and from that deduce

the cause of a structure presently far from the sea. But what facts could Darwin 'pretend to adduce, as true elements of proof' ? The only ones, says Sedgwick: 'are the varieties produced by domestication, or the human artifice of cross-breeding.' But, objects Sedgwick, we are still speaking of varieties; however dissimilar the animals are in appearance 'they are of one species'. Human artifice has not produced new species, and left to themselves the animals in any case revert. Sedgwick is, of course, exactly right. The only positive evidence in any sense of the word is the varieties (not species) under domestication. Darwin begins his book with this. To it he adds not any observed laws, but a rationalistically deduced 'secondary consequence of supposed, or known, primary facts' (Sedgwick's letter): natural selection. Sedgwick does not deny that natural selection takes place, but he argues that it must have the raw material of organic development to work upon. Not only does Darwin have no observed facts concerning the source and nature of variations, but he expressly professes ignorance of them.<sup>97</sup> On most, in fact, of the complicated mechanisms Darwin calls into action to save his theory from implausibility, he professes ignorance of the workings.<sup>98</sup> Surely no one, not even T.H.Huxley himself, could have claimed in 1860 that Darwin's theory was inductively demonstrated ? So, suppose that it is a speculation or hypothesis ? Surely Sedgwick allows these to have a use in science ?

The answer is that he does, as we have seen, but that he specifies conditions. Firstly, speculation must not be mistaken for inductive truth. If it is, then fact will be bent to hypothesis instead of vice versa. Yet, in the passage already quoted, Darwin states: 'I am fully convinced of the truth of the views given in this volume.' Darwin does not suggest his theory speculatively, he is 'fully

convinced' of its actual truth. So Sedgwick objects:

'Many of your wide conclusions are based upon assumptions which can neither be proved nor disproved, why then express them in the language and arrangement of philosophical induction ?' Speculation on untestable hypotheses should be recognised as such - not disguised as inductive truth.

A further condition on hypotheses is that they should be in accord with known facts. What were the known facts ? Sedgwick admits that 'the evidence is scanty', but evidence nevertheless exists, and:

'So far as it is positive, it seems to me point blank against them. As we ascend in the great stages of the Palaeozoic series... we have in each a characteristic fauna; we have no wavering of species.'

The actual facts of geology are that species are almost always well defined. Darwin had explained this away by suggesting enormous time gaps between strata. Sedgwick is willing to allow 'millions or billions' of years on each epoch but:

'I see no proofs of enormous gaps of geological time... in those cases where there is a sudden change in the ancient fauna and flora.'

In other words, Darwin has to assume in order to save his hypothesis gaps which are against all evidence.<sup>99</sup> On both points (c) and (d) above, the Origin of Species fails miserably.

But what of Sedgwick's own theories of creation ? We remember his words concerning Chambers:

'I advance no dogmatic theory; I simply appeal to facts, and affirm that they do not suggest the theory of development. On the contrary, they oppose to it, in the present condition of our knowledge, and evidence as strong as can be expressed in words. The whole blame of a rash hypothesis rests with my

opponent. A theory is worse than nothing if it reflect not back the present condition of our knowledge...<sup>100</sup>

The facts as Sedgwick saw them, were that species appeared suddenly in the fossil records. He could see no analogous present process by which such a thing could be seen to be happening today. On the other hand he could see sound reasons on another level for believing in a Creator-God. The most logical explanation, therefore, to Sedgwick, was that new species were created by a process outside our present experience.

Two points need to be emphasized about Sedgwick's viewpoint. First, whilst it appeals to beliefs beyond the physical, it is in accord with (and nowhere contradicts) known physical laws. The evidence is that appearance of species is beyond present physical explanation, and that no hypothetical 'time gaps' exist, and this is exactly what his theory assumes. Secondly, however, it is a form of explanation which a 'materialist' would find unacceptable. A 'materialist', in this context, is one who concentrates exclusively on material causes, and forgets or denies the larger framework of beliefs within which Sedgwick believed science to fit. A materialist, then, would be forced to accept some kind of theory of evolution - however much its 'explanations make demands on our credulity, that are utterly beyond endurance.' The fact that Darwin and his associates are willing to believe so improbable a theory indicates to Sedgwick an underlying materialistic thinking.

Finally, concerning the undulatory theory of light, Sedgwick noted elsewhere that it is 'considered, by some of those who have most deeply studied it, as well established as the theory of gravitation.'<sup>101</sup> Now Sedgwick was not a physicist, and Darwin was not only no physicist but was generally suspicious of mathematicians and physicists.<sup>102</sup>

But even a brief consideration reveals differences between the undulatory theory of light and the Darwinian theory of evolution in the 1860's. The undulatory theory had been put forward to explain known facts about the behaviour of light. It had not contradicted any known facts, and it had suggested further work to test it, as done in the work of Young and Fresnel. Sedgwick has not objected to theories or hypotheses per se, but to **any which**, like Darwin's, contravene his conditions for their use so clearly.<sup>103</sup>

Sedgwick was not 'highly inconsistent' in his application of scientific method.<sup>104</sup> Certainly there were tensions within his methodology, but (whatever we think of its deficiencies with present hindsight) his comments on Darwin's theory are essentially consistent with his modified Baconian inductive approach to **understanding science**.

## 7.5 Notes

1. e.g. Richard Yeo: 'Scientific Method and the Image of Science 1831-1891' in McCleod and Collins, Parliament of Science; M. Ruse, 'Darwin's Debt to Philosophy', Stud. Hist. Phil. Sc., 1975, 6, 159-181 and 'Darwin and Herschel', Stud. Hist. Phil. Sc., 1978, 9, 323-331; P. Thagard, 'Darwin and Whewell', Stud. Hist. Phil. Sc., 1977, 8, 353-56; D. Hull, 'Charles Darwin and Nineteenth Century Philosophies of Science' (in Giere, Theories of Scientific Method in the Nineteenth Century); E. Manier, The Young Darwin and His Cultural Circle also makes some comment (p. 51 etc.).
2. John Herschel, A Preliminary Discourse on the Study of Natural Philosophy, para. 74, page 83-4.
3. Ibid., p. 72.
4. Ibid., p. 104, also see p. 114.
5. Ibid., p. 271.
6. Ibid., p. 287.
7. 1st Edn. Discourse, p. 84; There are also specific points of non agreement, e.g. Sedgwick regards 'Hook' (sic, 3rd Edn. Discourse p. 98) as fairly unoriginal, whilst Herschel sees him as 'almost the rival of Newton' (p. 116).
8. e.g. 5th Edn. Discourse cccxxxiii; also Sedgwick's review of the Vestiges (p. 19) and his Presidential Address to the London Geological Society in 1831 (p. 299).
9. The Introduction to his History of the Inductive Sciences makes this clear, as well as outlining his philosophical ideas. Whewell in fact, saw the two as intimately related. In this respect, as in some others, as his work relates to us today his approach has

- more affinities with Kuhn than with Popper.
10. This temptation is increased by the availability of much of Whewell's writings through the ten volume set edited by G. Buchdahl and L.L. Laudan. It may be little exaggerated to suggest that since the 1960's there has been some renaissance of appreciation of Whewell amongst scholars. In this connection the appearance in 1968 of R.E. Butts, William Whewell's Theory of Scientific Method may be seen as a landmark.
  11. Whewell, The Philosophy of the Inductive Sciences (3rd Edn.), p. 17.
  12. Ibid., p. 18. The aphoristic style, of course, also reflects Kant. The exact extent of his reliance on Kant has long been disputed, and some of the main points were succinctly summarized by Ducasse in Madden (Ed.), Theories of Scientific Method, p. 183-184. Certainly Kant's influence was strong, but never followed slavishly.
  13. Whewell (ref. 11), p. 22-23.
  14. Ibid., p. 25.
  15. Ibid., p. 25. This was really at the centre of his controversy with Mill over Kepler; on the same page we read: 'When we see a body move, we see it more in a path or orbit, but this orbit is not itself seen; it is constructed by the mind.'
  16. Ibid., p. 37.
  17. Ibid., p. 40.
  18. Ibid., p. 52.
  19. In twentieth century philosophers of science obvious figures are P. Duhem (The Aim and Structure of Physical Theory), N.R. Hanson

(Patterns of Discovery), and T.S. Kuhn (The Structure of Scientific Revolutions).

20. Of the twentieth century figures cited, of course, Duhem arrived at the apparently startling conclusion that major paradigms were 'conventions' rather than 'truths'. Kuhn saw the impetus of his ideas towards instrumentalism, but seemed to dislike such a conclusion.
21. Whewell (ref. 11), p. 46.
22. Ibid., p. 66.
23. Whewell, History of the Inductive Sciences (3rd Edn.) Additions ch. 3, p. 344. On Bacon he says: 'we shall find his remarks on the Greek Philosophers very instructive.'
24. Whewell, The Philosophy of the Inductive Sciences, 1, p. 50.
25. Sedgwick, 'Review of the Vestiges', Edinburgh Review, July 1845, p. 2.
26. Sedgwick, 5th Edition Discourse, p. cxcix.
27. 5th Edition Discourse, p. cclxxxv.
28. Ibid., p. cxcix.
29. 1st Edition Discourse, p. 41.
30. The arch High Churchman Henry Phillpotts of Exeter wrote to him in 1843 wishing success for the 'admirable scheme of Examination of Theological students which has recently been established in the University of Cambridge.' He added: 'Shall I be forgiven if I take the liberty of further saying when I consider who it is who has thus conferred with me on their interesting subjects, that I hail with thankfulness this consecration of the highest attainments in Science to the service of God and of His Church ?' (Add Ms a 53 15, Whewell collection, Trinity College).



31. Various articles on this Whewell-Mill controversy have appeared, e.g.: E.W. Strong, 'Whewell vs J.S. Mill on Science', Journal of the History of Ideas, 1955, 14, 209-231; H.T. Walsh 'Whewell and Mill on Induction', Journal for the Philosophy of Science, 1962, 279-284; and Curtis Wilson 'Newton and Some Philosophers on Kepler's "Laws"', Journal of the History of Ideas, 1974, 35, 231-258. The consensus that Mill did not really understand Whewell's position is surely correct. It is, however, arguable that the modern commentators (and especially Walsh) do not fully grasp it either. The latter summarises Whewell's views as that '(Things) have ... various non relational properties, such as colour, roughness, and the like. These things affect us through our sensory apparatus...' (p. 279). A detailed discussion, however, of whether this really does justice to Whewell's position is beyond the scope of the present thesis. Sedgwick's comments on Kepler's methods (5th Edition Discourse, p. ccii) are, as one might expect, at a different level from Whewell's. He recognises Kepler's 'imagination', but sees him conventionally as making 'his ideal notions bend to his discoveries'. The Whewellian perspective is wholly absent.
32. Butts, in William Whewell's Theory of Scientific Method, P.vii, notes his dominance in his own times but that his philosophical influence 'appears almost to have died with him.' C.S. Peirce was alone in valuing his work more than Mill's, and only recently has Whewell's work really begun to receive its deserts.
33. The reference here is to ideas already mentioned in note 20 above.
34. 1st Edition Discourse, p. 3.
35. 1st Edition Discourse, p. 9 (and all Editions).

36. 1st Edition Discourse, p. 144.
37. Sedgwick quotes in full the relevant passage from Newton's Opticks.
38. The Athenaeum, October 5th, 1844.
39. The defects of Hume's associationist answer, Mill's answer in his System of Logic, and later answers e.g. of Logical Positivists, illustrate the problems of non 'theologically based' assertions of a law of uniformity.
40. The latter is in the Macvey Napier Papers in the British Library, Passmark ADD 34625.
41. Sedgwick (ref. 25), p. 2.
42. Ibid., p. 4.
43. Ibid., p. 10-11.
44. Ibid., p. 23.
45. Ibid., p. 28.
46. Ibid., p. 49.
47. See e.g. 5th Edition Discourse clxviii, 118, 133, 142, 230.
48. Ibid., xxii.
49. Ibid., xxvii-xxviii.
50. Ibid., xlvii-xlvi.
51. Ibid., cxli.
52. Ibid., liii.
53. Ibid., ccxxxv-ccxxxvi.
54. Ibid., lxxi.
55. Ibid., lii.
56. Ibid., cciii.

57. Ibid., lxxxviii.
58. Ibid., lxi.
59. Ibid., p. 217.
60. Ibid., ccxliv.
61. Ibid., cccxxxvi.
62. Ibid., p. 101.
63. Levere, Poetry Realised in Nature, p. 92.
64. Ibid., p. 143.
65. See e.g. 5th Edition Discourse, cclxviii, cclxvi, etc.
66. Levere (ref. 63), p. 206 (see also p. 37 etc.). David Knight  
(The Transcendental Part of Chemistry, p. 113) has also seen  
ways in which Coleridge was indebted to Schelling. Muirhead  
(Coleridge as Philosopher) also has a chapter on Coleridge's  
'Philosophy of Nature'.
67. 5th Edition Discourse cclxiv.
68. Ibid., cclxxxiv; the 'followers of Kant' are, in Sedgwick's  
opinion, pantheists.
69. Ibid., cci, cciv, cclxxii, 184, 222, 283.
70. Ibid., p. 270.
71. Ibid., p. 125-127.
72. Ibid., p. 214.
73. Ibid., p. 216.
74. Ibid., p. 217.
75. Whewell, Of a Liberal Education, p. 224, etc.

76. 5th Edition Discourse cccxxvii. In a letter to Whewell (June 2nd 1849) Sedgwick gives his support for the new scheme, though he does want some flexibility. He suggests that the ordinary examination might be a viva at the discretion of the Professor, though the Tripos should be by papers. (Trinity Coll. Add Ms.A63/81)
77. D.B. Wilson, 'Experimentalists amongst the mathematicians: Physics in the Cambridge Natural Science Tripos: 1851-1900', HSPS, 1982, 12, 325-371, especially p. 327.
78. Roy Porter, 'The Natural Science Tripos and the Cambridge School of Geology 1850-1914', History of Universities, 1982, 2, 193-216.
79. Porter (ibid.) cites the 1851 and 1857 papers as particularly exemplifying this (p. 206 and note 62).
80. Mary Hesse ('History and Philosophy of Science in the Early Natural Science Tripos', Cambridge Review, November 24th 1962, 140-45) traces the decline in philosophical content in the Cambridge Natural Science Tripos after the waning of Whewell's influence. The actual 'success' of these Tripos in student career terms is dealt with in R. MacLeod and R. Moseley, 'The "Naturals" and Victorian Cambridge: Reflections on the anatomy of an elite 1851-1914', Oxford Review of Education, 1980, 6, 177-195.
81. F. Darwin, The Life and Letters of Charles Darwin, 1, p. 185.
82. Ibid., p. 56.
83. See P.H. Barrett, The Collected Papers of Charles Darwin, 1, p. 16 and 18.
84. F. Darwin (ref. 81), 1, p. 67; Sedgwick also wrote on November 7th 1835 to Dr Butter (a letter in the British Library, Pressmark ADD 34589) with a P.S. concerning the Shropshire and North Wales Natural

History Society, adding: 'I suppose my friend Dr. Darwin is a member' and predicting that Charles 'will have a great name among the naturalists of Europe.'

85. F. Darwin (ref. 81), 2, p. 125.
86. Origin of Species (not removed even from the 6th edition - p. 456).
87. Hull, Darwin and His Critics, says: 'After abandoning Wernerian geology, Sedgwick remained highly conservative throughout the rest of his life ...' (p. 127).
88. Clark & Hughes, 2, p. 440.
89. Extracts of this letter are printed in Clark & Hughes, 2, p. 358, and it is given in full in F. Darwin (ref. 81), 2, p. 247-50 and in D. Hull, Darwin and His Critics, p. 157-58. All give its date as 24th December 1859. This date, however, would imply that Sedgwick (according to the letter) was lecturing in Cambridge on the 23rd December (though intending to be in Dent over Christmas, and presumably without students), and that Darwin replied to the letter on the 26th November, exactly a month before it was written. The clearly written date on Darwin's letter to Sedgwick would appear to be the correct one, and the date printed for Sedgwick's an error, the true date being 24th November.
90. An extract of this letter is given in F. Darwin (ref. 81), 2, p. 251, but this passage is omitted. The letter is in the British Library.
91. The same expression is used in two letters in F. Darwin, (ref. 81), (to Lyell, 24th March 1860; to Asa Gray, 3rd April 1860). It is also used in two further letters in F. Darwin, More Letters of Charles Darwin (to W. Miller, 1st December 1859; to T.H. Huxley,

25th November 1859).

92. Clark & Hughes, 2, p. 361, (dated 28th March).
93. Spectator, 24th March and 7th April.
94. N. Barlow, Darwin and Henslow, Letter 114 (May 14th 1860).
95. D.L. Hull 'Charles Darwin and Nineteenth Century Philosophies of Science', in Foundations of Scientific Method: The Nineteenth Century (Ed. R.N. Giere and R.S. Westfall), p. 122.  
Hull is criticising A. Ellegard's Darwin and the General Reader.
96. Hull (ref. 87), p. 169.
97. We should: 'acknowledge plainly our ignorance of the cause of each particular variation' (Origin of Species, 1st Edn. p. 173, 6th Edn. p. 128). 'Our ignorance of the laws of variation are quite profound. Not in one case out of a hundred can we pretend to assign any reason why this or that part differs, more or less, from the same part in the parents.' (1st Edn. p. 202; 6th Edn. p. 154).
98. Darwin admits we do not know why hybrids are infertile (1st Edn. p. 276, 6th Edn. p. 270), why sexual elements of species have become modified to make them mutually infertile (6th Edn. p. 290, not in 1st Edn.), why we find no records of the swarming living creatures which must have populated primordial periods (1st Edn. p. 313, 6th Edn. p. 315), what conditions are most favourable for the multiplication of new and dominant species (1st Edn. p. 328, omitted from 6th), 'how profoundly ignorant we are with regard to the many and curious means of occasional transport' (1st Edn. p. 393, 6th Edn. omits 'profoundly'), and the causes of new features arising in embryos (1st Edn. p. 426).

99. Though few practising biologists today would doubt evolution, the continuing failure to find evidence for the large time gaps in the geological record which Darwin wanted, led Niles Eldredge and Stephen Jay Gould to formulate their theory of punctuated equilibria - evolutionary change in a series of jumps. For some general references see the report of a historic Chicago Conference (Science, 21st November 1980, 210), and for a particular application, Tom Kemp, 'The Reptiles that Became Mammals', New Scientist, 4th March 1982, 93, 581-584.
100. 5th Edition Discourse, lxxi.
101. Ibid., p. 103. ,
102. See F. Darwin, More Letters of Charles Darwin, 1, p. 134-5.
- 103..Interesting, concerning hypotheses, is the 'Glen Roy' episode. In 1838 Darwin spent some days examining the apparent 'parallel roads' along the sides of Glen Roy, and in 1839 published 'Observations on the Parallel Roads of Glen Roy'. (Phil. Trans Roy. Soc. Lon., 1839). He suggests they are raised beaches, rejecting the idea of an inland lake. The Royal Society submitted the paper to Sedgwick as a referee, and his report is printed as Appendix B in Rudwick's, 'Darwin and Glen Roy: A "Great Failure" in Scientific Method?', (Stud. Hist. Phil. Sci., 1974, 5, 98-185). Sedgwick remarks that it: 'contains much original research, much ingenious speculation, and some new and very important conclusions.' Sedgwick recommends printing it - his criticisms are almost all stylistic ones. Sedgwick had no objection to 'speculation' provided (as Darwin's made its case) it was based on facts and did not actually conflict with other known facts - and provided too that it was recognised as part

speculation. Later, in 1847, Darwin stuck to his theory against a glacier theory in a letter never published (given in Paul Barrett, 'Darwin's "Gigantic Blunder"', Journal of Geological Education, 1973, 21, 19-28). Later, he finally had to admit that his theory was wrong and the glacier theory correct - seeing his early paper as a 'great failure'. Rudwick's paper contains a very useful discussion of the various methodological and psychological pressures operating in the incident. From our present viewpoint there are two interesting corollaries. First, it shows Sedgwick was not adverse to speculation - and from Darwin at that - provided it was of a particular kind. Secondly, it shows that Darwin could (from Sedgwick's viewpoint) be in too much haste to dogmatise over hypotheses - and later turn out to be completely on the wrong track. When, with hindsight, we see Darwin as one of the 'super heroes' of science, he may become invested with an aura of scientific surety. But he may not have appeared so to his contemporaries. Could not the Origin of Species have turned out as the Glen Roy paper ?

104. Hull, (ref. 87), p. 169. Poor Fleeming Jenkin has even worse treatment. His review is actually a model of scientific caution, but in it he rightly points out that the evolutionary theory has little positive evidence, needs a lot of ad hoc assumptions, and is in direct conflict on the time issue with apparently well inductively based and tested physical laws. For this, Hull places him with the 'arrogant physicists' on p. 349.



## 7.6 Sedgwick's Ideas on Revelation and Geology

### 7.6.1 Science, Religion and Materialism

A major theme of this thesis has been the interplay of religious and scientific ideas. This final chapter will contain a certain amount of recapitulation and drawing together of themes, but will also look more specifically at the actual relationship as perceived by Sedgwick between Genesis and his geology.

Scientific methodology might be affected by religious ideas in the following particular ways:

- (i) By a mixing of 'moral'<sup>1</sup> and physical concepts. This could be:
  - (a) by using teleological and moral arguments to decide what physical structures 'must be' (arguing from moral to material)
  - or (b) by 'reduction' of moral concepts to material ones (arguing from material to moral)
- (ii) By assuming that Divine revelation (i.e. the Bible) could actually teach us scientific facts.

All three of these were unacceptable to Sedgwick, but unfortunately he did not always separate them clearly from other issues. The matters are also further complicated by the fact that (i)(b) is not at all the same issue as that of revelation. The materialism of (i)(b) effectively denied the reality of a 'moral' sphere, perceived in a Sedgwickian dualism; yet that moral sphere was not known only through Scripture but also through the natural theology of conscience. More was at stake, in Sedgwick's view, than Biblical literalism - or even than Christian doctrine. Thus, whilst (i)(b) did conflict with Scripture as Sedgwick saw it, it also conflicted with a belief in the reality of a whole sphere of human experience.

Further complication arises because to Sedgwick the term

'materialism' implied more than one aspect. The fundamental meaning of the term is a denial of ultimate reality to anything other than the material, but it has two facets:

- (1) A reductionism of type (i)(b) above - regarding 'moral' phenomena as by-products of material causes. Effectively this denies ultimate reality to human experiences of will, volition, right and wrong.
- (2) A denial of 'final cause', or ultimate design behind the universe. Effectively this denies any reality to Divine will, volition, and legislation of right or wrong for a created world.

These two facets are explicitly linked by Sedgwick in the passage quoted above on p.243 of this thesis. That passage also makes clear the fact that early defenders of spontaneous generation and evolution (in Sedgwick's view) were all materialists in both aspects of that term. But he nowhere says that they were 'rank materialists' because they believed in evolution. Evolution is a necessary part of materialism, but does not necessarily imply it. Thus Sedgwick immediately continues:

'But a doctrine may be true and yet may be turned to evil purposes.'<sup>2</sup>

Sedgwick nowhere implies that evolution in itself conflicts either with the concept of final cause or with Biblical revelation.<sup>3</sup> Both his reviews (of Vestiges and of Origin of Species) reject the doctrines of evolution as 'little better than a phrensied dream' on scientific and geological grounds. Nevertheless, Sedgwick is suspicious of evolutionists, because of the association of the doctrine with materialism. He does, moreover, detect different

aspects of materialistic thinking behind both books. The Vestiges did accept final causes, and saw a Creator as behind all the development; but, on the other hand, it very clearly adopted a materialistic reductionism - as described in point (1) above. As we have seen, in both his review and in the lengthy comments in the 5th Edition Discourse, Sedgwick focussed mainly on this reductionism in his moral indignation against the Vestiges. He attacked the evolution scientifically because he believed it to be scientifically untenable and he wished to remove the cloak of apparent scientific respectability from a schema he regarded as pernicious and materialistic. But the evolution was not the essence of what made Vestiges a 'dish of rank materialism'.

Chambers had shown the facet of materialism shown in (1). Darwin, in contrast, did not make any significant comment in the Origin of Species on this kind of reductionism. On the other hand, Darwin did seem, to Sedgwick, to be showing signs of (2) and denying final cause. So Sedgwick wrote to him:

'There is a moral or metaphysical part of nature as well as a physical. A man who denies this is deep in the mire of folly.

'Tis the crown and glory of organic science that it does through final cause, link material and moral; and yet does not allow us to mingle them in our first conception of laws, and our classification of such laws... You have ignored this link; and, if I do not mistake your meaning, you have done your best in one or two pregnant cases to break it...'<sup>3</sup>

Sedgwick puts this more strongly in his review: He 'utterly detests' it, (original version) or has a 'deep aversion' (revised version) to the theory, because:

'... it utterly repudiates final causes, and thereby indicates a demoralised understanding on the part of its advocates. By the

word 'demoralised' I mean the want of capacity for understanding the force of moral evidence, which is dependent on the highest faculties of our nature. What is it that gives us the sense of right or wrong, of law, of duty, of cause and effect? What is it that enables us to construct true theories on good inductive evidence?... By gazing only on material nature a man may easily have his very senses bewildered... he may become so frozen up, by a too long continued and exclusively material study, so as to lose his relish for moral truth...'<sup>4</sup>

There are two basic issues here. Firstly, Sedgwick reasserts the reality of the 'moral' sphere. If we are no more than highly developed animals, then our sense of right and wrong, duty, God, and indeed our belief that our minds can comprehend truth, **are presumably all a 'cheat'**. Sedgwick asserts that we cannot speculate on man's position in nature if we 'keep his highest faculties out of our sight.' It is not that **evolution** in itself would imply this (though naturally Sedgwick does not state this strongly). It is that he detects a materialist undertone in the work. One aspect of this was explored, for example, in part 7.5.6 of this thesis: the materialist refusal to consider explanations other than material ones. But there may be another aspect of it - an indication that Darwin has perhaps 'gazed too long on material nature', **he has lost** his sense of wonder and what we might call 'God-consciousness'. Sedgwick, in his Discourse, had recommended a study of the Newtonian philosophy because:

'It teaches us to see the finger of God in all things animate and inanimate.'<sup>5</sup>

This did not mean that Sedgwick inserted pious pronouncements in the middle of sober scientific accounts, but it did mean a certain sense

of wonder at nature. He evidently finds it lacking in Darwin's Origin of Species, and illustrates this by referring to Darwin's 'natural selection' explanation of the hive bees' skill in construction of honeycomb. Not only does Sedgwick find Darwin's actual explanations incredible, but he adds in his letter that the passage has offended his 'moral taste'. Darwin, for example, had written:

'By such modifications of instincts which in themselves are not very wonderful ... I believe that the hive-bee has acquired, through natural selection, her inimitable architectural powers.'<sup>6</sup>

But to Sedgwick it was all very wonderful, and he could see the hand of a Creator behind it whatever the mechanism. In point of fact he finds Darwin's suggestion physically incredible, but even if he had not:

'Take the case of the bee cells. If your development produced the successive modification of the bee and its cells (which no mortal can prove), final cause would stand good as the directing cause under which the successive generations acted and gradually improved.'<sup>7</sup>

Evolution itself would not says Sedgwick - even if by natural selection - necessarily negate final cause. But Darwin, to whom it was apparently not very wonderful, has no vision of it. Yet Darwin does not realise (Sedgwick would have said) that once the theistic framework is removed, the rational base for all that is truly human (including confidence in our inductive powers) will crumble with it.

Sedgwick, then, was not objecting to evolution per se, as materialistic, though he sees Darwin's book as so. Darwin lacked the intense 'God-consciousness' of a Sedgwick (or a Miller, a Kingsley, or even a Gray). There has been considerable debate both over whether Darwin was or was not actually a theist when he wrote the Origin, and over whether he perceived natural selection as excluding design.<sup>8</sup>

If natural selection is said to be by 'chance' is this word being used inductively and scientifically or metaphysically ? To Sedgwick, in any case, the word had no meaning when applied to the supreme prescient Intelligence of God;<sup>9</sup> Darwin's contemporary supporters did not all agree on it, and neither do modern evolutionary biologists.<sup>10</sup> In any event, it is this question which makes Sedgwick see the Origin as a 'dish of rank materialism' - not the evolution as such. As we have seen, God could very well have directed a process of bee evolution, so presumably the same applies to a whole process of organic development.

Now that we have reached this point in our recapitulation it may be well to set out in a set of short statements the various aspects of Sedgwick's views on these related issues. Most have already been dealt with, but putting them together may help (in the face of the various modern writers who accuse Sedgwick of various forms of absurd inconsistency) to demonstrate his basic consistency:

- (A) Teleological arguments from final causes may have some use in biology, but none in physics or geology.<sup>11</sup>
- (B) Experience of the moral and volitional sphere cannot be reduced to the material.<sup>12</sup>
- (C) Though 'moral' and 'material' cannot be mixed at the level of immediate cause (as in (A) or (B)) yet they may ultimately be linked 'at the top' since reason finally leads us to believe that the same personal divinity experienced in the moral and personal sphere is the person who has designed and who sustains the physical laws. Thus knowledge is ultimately unified.<sup>13</sup>
- (D) Though natural theology (working both from the conscience and from physical observation) indicates a Creator-God, it cannot

teach us the details of God's way of redemption. These are given us by revelation in the Bible.<sup>14</sup>

(E) The Biblical revelation is a moral and spiritual guide, and is not intending to teach us physical truth. The latter rests on observation and induction, not revelation.<sup>15</sup>

(F) Nevertheless, since it is the same God who both wrote the Bible and created the world we may expect the two sources of knowledge ultimately to be in harmony.<sup>16</sup>

As this thesis has already emphasized, many of these points are in line with the tradition established by Francis Bacon, and Sedgwick often explicitly refers to him. Thus, e.g., on point (A):

'Of organised beings we know the beginning and the end, and we know the leading purposes to which their organs are subservient. Hence, in speculating about the functions of organic structure we may often use the doctrine of Final Cause as the foundation of our reasoning and the source of true induction. This we cannot do in questions that are purely physical: for while we contemplate any great physical law we neither know its beginning nor its end; neither do we comprehend its whole purpose. Thus, while analysing the properties of light by direct experiment, we should only desert the true road to discovery were we to turn aside to consider the adaptation of light to our wants; or to the anatomy of the eye, and its fitness to convey the impressions of light to the visual sense. Bacon saw this distinction clearly, and wrote well upon the misapplication of final causes... "The handling of final causes, mixed with the rest in physical enquiries, hath intercepted the severe and diligent inquiry of all real and physical causes, and given men the occasion to stay upon these

satisfactory and specious causes, to the great arrest and prejudice of further discovery..."<sup>17</sup>

The exception made for biology is perhaps in reference to the work of Cuvier and those who followed him. In such work, the assumption is made that each organ has a function, and this assumption has proven fruitful rather than sterile to scientific discovery.

Does Sedgwick violate his own principle in his comments on evolution and creation? He would be violating (1) if he were to argue that because the world was made to God's design therefore we know a priori that species must appear suddenly by creation and without any genetic descent. But I know of no place in which Sedgwick makes any such assertion. Rather, he claims that the sudden appearance of new species by creation is a fact known by strictly scientific induction from the fossil record. After having failed on a scientific basis to see any way of accounting for such phenomena on the basis of present known scientific laws, then Sedgwick turns to a higher level of knowledge and claims (true to his strictly empiricist theology) that there is no a priori reason - given a creator - why that creator should not have used a different principle of action at different times. It is Chambers, not Sedgwick, who makes pronouncements on what God 'must have' done; and it is Darwin, not Sedgwick, who argues a priori for the truth of natural selection.

On point (D), as on a number of other issues<sup>18</sup>, Sedgwick follows his Mentor Bacon fairly directly:

'But if the Bible be a rule of life and faith - a record of our moral destinies - it is not (I repeat), nor does it pretend to be, a revelation of natural science. The credibility of our religion depends on evidence internal and external. Its



internal evidence is seen in the coherence of its design from its first dawning to its day spring from on high<sup>19</sup> - in its purity and moral dignity - in its exalted motives fitted to call forth man's highest moral and intellectual energies ... Its external evidence mingles itself in a thousand ways with the internal- but finally resolves itself into the strength of human testimony, proving that God has at many times made a visible manifestation of his power on earth; promulgating among mankind a rule of like, enforcing it by the terror of penal sanctions, and confirming it by miracles publicly wrought in attestation of its truth. Physical science on the contrary, derives no support from internal evidence or external testimony; but it is based on experiment alone, is perfected by induction, and is drawn out into propositions by a rational logic of its own. To confound the ground works of philosophy and religion is to ruin the superstructure of both: for the bases on which they stand, as well as their design, are absolutely separate; and we may assume it as an incontrovertible truth, that the inductions of philosophy can no more be proved by the words of revelation, than can the doctrines of Christianity be established by the investigations of natural science.<sup>20</sup>

'... if there be religious truth, as there assuredly is, it must be in its essence different from physical truth, and must rest on a different foundation: - and that if there be, as there is, a real demonstrative body of physical truth, it must also rest on its own foundations, which are in no wise to be confounded with moral evidence. But truths,

though distinct, are not therefore in conflict; and though resting on different foundations they may give help and support to one another.<sup>21</sup>

This was written in 1849, and it essentially agrees with his views in 1825:

'The authority of the sacred records has been established by a great mass of evidence at once conclusive and appropriate; but differing altogether in kind from the evidence of observation and experiment by which alone truth can ever be established. It must, therefore, at once be rash and unphilosophical to look to the language of revelation for any direct proof of the truths of physical science. But truth must at all times be consistent with itself. The conclusions reached on the authority of the sacred records may, therefore, consistently with the soundest philosophy, be compared with the conclusions established on the evidence of observation and experiment; and such conclusions, if fairly deduced, must necessarily be in accordance with each other ...'<sup>22</sup>

Sedgwick was explicitly following Bacon when he viewed revelation and science as based on different foundations, and involving different approaches to the discovery of truth. Yet, as the last quoted sentence shows, he also followed Bacon in believing that the two would ultimately be in harmony:

'It is an assured truth, and a conclusion of experience that a little or superficial knowledge of philosophy may incline the mind of man to atheism, but a further proceeding therein doth bring the mind back again to religion: for in the entrance of philosophy, when the second causes which are next unto

the senses, do offer themselves unto the mind of man, if it dwell and stay there. it may induce some oblivion of the highest cause; but when a man passeth on farther, and seeth the dependence of causes, and the works of Providence, then according to the allegory of the poets, he will easily believe that the highest link of Nature's chain must needs be tied to the foot of Jupiter's chair. To conclude, therefore, let no man upon a weak conceit of sobriety, or an ill-disposed moderation, think or maintain that a man can search too far, or be too well studied in the book of God's word, or in the book of God's works; divinity or philosophy; but rather let men endeavour an endless progress or proficence in both; only let men beware that they apply both to charity, and not to swelling; to use and not to ostentation; and again, that they do not unwisely mingle or confound those learnings together.<sup>23</sup>

Consistently, throughout his life, Sedgwick reaffirmed that there was nothing to fear from advancing knowledge. The knowledge produced by true scientific induction would always ultimately harmonize with the right interpretation of Scripture:

'No opinion can be heretical but that which is not true.

Conflicting falsehoods we can comprehend- but truths can never wage war against each other. I affirm, therefore, that we have nothing to fear from the results of our enquiries, provided they be followed in the laborious but secure road of honest induction.<sup>24</sup>

#### 7.6.2 Sedgwick on Revelation, Creation and the Flood

Sedgwick, as we have seen, was confident that the findings of science would ultimately harmonise with revelation. How, then, did he himself see the harmony of the Biblical accounts of (i) the

creation, and (ii) the Noachic flood, with scientific findings ?

Before commencing consideration of these issues, it needs to be said that the material for determining Sedgwick's views on them is scanty. He usually refused to commit himself on any kind of detail, both in published work and in private letters. Nevertheless what materials there are may be set out here.

To begin with the creation issue, there are actually two questions of interest. One of these concerns the way in which the Genesis creation accounts might be harmonised with the geological records as Sedgwick understood them. The other concerns whether, in his view, Genesis contradicted evolution as such.

Before ever Sedgwick began his geological work, geology had begun to suggest great periods of time before man appeared on the earth. As we have already seen<sup>25</sup> the two main ways in which harmony with Genesis was sought, were forms of the 'gap-theory' and the 'age-day' theory. In his 1st Edition Discourse in 1833, Sedgwick wrote:

'The Bible instructs us that man, and other living things, have been placed but a few years upon the earth; and the physical monuments of the world bear witness to the same truth. If the astronomer tells us of myriads of worlds not spoken of in the sacred records; the geologist in like manner proves (not by arguments from analogy, but by the incontrovertible evidence of physical phenomena) that there were former conditions of our planet, separated from each other by vast intervals of time, during which man, and the other creatures of his own date, had not been called into being. Periods such as these, belong not, therefore, to the moral history of our race; and come neither within the letter nor the spirit of revelation. Between the first creation of the earth and that day in which it pleased

God to place man upon it, who shall dare to define the interval ?

On this question Scripture is silent: but that silence destroys not the meaning of those physical monuments of his power that God has put before our eyes...'<sup>26</sup>

Sedgwick then goes on to say that whatever difficulties there may be in geology, the answer is not to fabricate a fanciful 'Scriptural geology' but to stick to inductive facts, sure in the knowledge that ultimately scientific and theological truths will harmonize. But the actual schema put forward here is the gap-theory. It depends upon two assumptions. The first is that man is relatively recent (which Sedgwick assumes), and the second is that some kind of catastrophe separates the periods of previous fauna from those of today. In this case, Sedgwick argues, just as the Bible tells us nothing about beings on other worlds (which from analogy based on astronomical discoveries we may believe exist), so it tells us nothing about beings or fauna prior to our own era. This was much in line with Evangelical thinking on the issues, and the passage closes with a word from Chalmers (who espoused the gap-theory) that Christianity has nothing to fear and everything to hope for from the advance of 'philosophy' (i.e. of science).

Later in his Discourse Sedgwick added the comment:

'Another indiscretion (far different however from the egregious follies I have just noticed<sup>27</sup>) has been committed by some excellent christian writers on the subject of Geology. They have not denied the facts established by this science, nor have they confounded the nature of physical and moral evidence: but they have prematurely (and therefore, without an adequate knowledge of all the facts essential to the argument) endeavoured to bring the natural history of the earth into a

literal accordance with the book of Genesis - first, by greatly extending the periods of time implied by the six days of creation (and whether this may be rightly done is a question only of criticism and not of philosophy) - and secondly, by endeavouring to show, that, under this new interpretation of its words, the narrative of Moses may be supposed to comprehend, and to describe in order, the successive epochs of Geology. It is to be feared that truth may, in this way, receive a double injury; and I am certain that the argument, just alluded to, has been unsuccessful. The impossibility of the task was however (as I know by my own experience) a lesson hard to learn: but it is not likely again to be attempted by any good Geologist. The only way to escape from all difficulties pressing on the question of cosmogony has been already pointed out. We must consider the old strata of the earth as monuments of a date long anterior to the existence of man, and to the times contemplated in the moral records of his creation. In this view there is no collision between physical and moral truth. The Bible is left to rest on its appropriate evidences, and its interpretation is committed to the learning and good sense of the commentator: while Geology is allowed to stand on its own basis, and the philosopher to follow the investigations of physical truth, wherever they may lead him...<sup>28</sup>

Sedgwick, then, is highly critical of the age-day theory, in favour of a gap-theory. Which of the two theories was 'really' more exegetically and geologically convincing in the period is not our concern. But Sedgwick seems to favour the gap-theory because it (for the time being at least) left him free to take a very Baconian

approach, with both exegesis and geology each resting on their separate bases, neither interfering with the other. The age-day theory, on the other hand, requires the expositor to read 'yom' (day) as a long time period, and requires the geologist to find the actual epochs to match those of Genesis.

Unfortunately even the erudition of a Sedgwick cannot make this sound plausible. The gap-theory equally requires the expositor to allow a large gap somewhere in Genesis 1.1-2, and (presumably) to allow 'tohu and bohu' (without form and void) to imply a derivative state of catastrophe. On the other hand it tells the geologist that he must expect ultimately to find traces of a world-wide catastrophe and complete new start to fauna. It is pure wishful thinking to see the gap theory as leaving science and revelation as independent.

In the mid 1840's we have a glimpse that Sedgwick is holding to the same views. One side of a correspondence is extant between Sedgwick and Richard Gwatkin, who also wrote to Whewell on scientific and religious matters. On 10th May 1842 Gwatkin wrote to Sedgwick:

'I did not misunderstand you to say that Moses could not be reconciled with the facts of Geology. You have expressed yourself quite well on this point, viz that all collision is to be avoided by placing the Mosaic account at the end of the geological period instead of considering it as you once thought to be descriptive of that period. Your opinion is so strongly expressed as to be enough to stagger a mere book geologist who inclined to the latter view. On that account I felt a wish that you had more fully expressed the grounds of your present position...'<sup>29</sup>

The latter wish is one which others of us may share. But we learn from this (and perhaps it is strongly hinted in Sedgwick's words

'as I know by my own experience' in the passage quoted above) that Sedgwick had formerly espoused the age-day theory himself. He was often the most vehement against views once held himself but later rejected.

In January 1843 Gwatkin wrote again, commenting on a tract on the age-day theory which he had published. Sedgwick has (he says) 'allowed much more than I was prepared to expect', though has said: 'every attempt must at present fail'.

According to Clark and Hughes it was 'soon after 1844' that Sedgwick wrote on the issues to 'a friend'. The imprecision and abridgement in this instance are irritating to us, since so little material is available on this aspect of Sedgwick's thinking, but in default of the original letter, the Clark and Hughes extract is produced below:

'The first two verses (of the first chapter of Genesis) are an exordium, declaring God the Creator of all material things; and I believe it means, out of nothing, at a period so immeasurably removed from man as to be utterly out of the reach of his conception. After the first verse there is a pause of vast and unknown length, and here I would place the periods of our old geological formations, not revealed because out of the scope of revelation. We are then told that 'the earth was without form and void, and darkness was upon the face of the deep.' Who can dare to say that he comprehends these short and mysterious words? They may, perhaps describe the condition of the earth after one of the many catastrophes by which its former structure had been broken up, and of which we can, on its present surface, find so many traces. But these are speculations. I value them not, for they are,



perhaps, worse than nothing. After the word 'deep' there is a pause. The work of actual present creation now begins. The spirit of God broods over the dead matter of the world, and in six figurative days brings it into its perfect fashion and fills it with living beings.

Why may He not have manifested His power while His spirit moved on the waters in ten thousand creative acts never revealed (because unconnected with the moral destinies of man), yet recorded in clear characters on stony tablets to be read and admired in after-times by the descendents of the last created being, to whom faculties were given whereby they might comprehend the laws of the material world, and rise from them to some faint, glimmering perception of their Creator's glory?<sup>30</sup>

In 1850 the fifth edition of his Discourse was published. In spite of Sedgwick's friendly correspondence with Hugh Miller from the mid-forties, and his evident respect for Miller as a geologist, the passages quoted from the earlier edition above were reprinted without change.<sup>31</sup> Since Miller was developing an age-day theory this may seem surprising.<sup>32</sup>

Elsewhere in the 5th Edition Sedgwick wrote:

'Revelation tells us that God created the heaven and the earth - that man was the last created of living beings - and that God then rested from His labours. Many learned heathens held that the order of nature, animate as well as inanimate, had been from eternity. Modern science ... proves that the the order of Nature has not been eternal, and that man is a creature of the last and latest period. Science also tells

us, that since the appearance of man, creative power in Nature appears to have been at rest. That there are difficulties in the interpretation of the opening words of the Book of Genesis, we do not deny. To bring them into a literal accordance with all the phenomena in the past history of Nature would imply, on our part, a perfect knowledge of the past history of Nature; but such a knowledge we have not. The progress of science may clear up these difficulties ..<sup>33</sup>

In 1858 he wrote on the subject of creation and the flood to Mrs Cropper of Lowestoft, who apparently had a clergyman friend preparing to give a short lecture course on these topics. Clark and Hughes again irritatingly abridge this letter, but a further part (also incomplete) is in a copy in the Sedgwick collection in Cambridge University. Sedgwick implies much caution - admitting that he himself would not like at this stage to give a public lecture on the subjects - lest he mislead. In this he refers specifically to Miller:

'Before your friend lectures on the subject, you might mention he ought to familiarise himself with Hugh Miller's last work. I believe he had corrected the concluding proof sheet at the time of his most melancholy death. It is a very clever work, written with perfect sincerity, and written with an ample knowledge of the leading facts and discoveries of Geology. Many persons think its reasoning satisfactory, I am compelled to say that I am not of their number. Many parts of it are, I think, far too imaginative and there are a myriad difficulties he has not overcome. Still, some parts of his work are excellent. In regard to my own opinions it would be foolish to say they had undergone no change since the discourse to which you allude was

published.<sup>34</sup>

The published letter states:

'Another opinion I formerly held was this: viz. that the modern was more distinctly separable from the anterior period than it proves to be on further investigation... I have no fear of the ultimate result, but we have ample work for another half century before we can be prepared to draw our lines of demarcation correctly, and till that is done I should think it premature to talk of comparing the geological days (or periods) with the Mosaic days. That this will be done one day I have very confident expectation, because we have already done much, though a few blots remain to be removed by the honest scrubbing-brushes of the rising generation of geologists...'<sup>35</sup>

In a footnote Clark and Hughes say: 'The omitted passage contains a discussion on the meaning of the days of creation. Sedgwick's views on this question have already been stated. See above pp.76-80'. This is extraordinary, for the Cropper letter contains two references to ways in which his views have changed since the Discourse (1850 ?).

The separation of the modern from the anterior period (essential to the gap theory one would think) proves less distinct than he had supposed. Moreover, the last sentence quoted above could be taken to imply that the word 'this' refers back to an age-day theory. Was Sedgwick saying that Miller had not yet succeeded but that one day some age-day theory would ?

I have not located any material to give further insight into Sedgwick's ideas and their development. In reality he leaves us with little concrete advice on harmonising Genesis and Geology. In 1868, aged 83, Sedgwick decided that man was 'of a far higher antiquity than that which I have hitherto assigned to him.'<sup>36</sup>

What, if any, difference this made to the gap-theory we are not told. All that we are left with is his hope of the future - one day we shall have knowledge to harmonize Genesis and Geology.

Concerning the flood, we saw in an earlier chapter<sup>37</sup> how Sedgwick changed his mind between 1825 and 1831 about its universal nature. We also considered the passage in his 1825 paper in which, true to his Baconian principles of separate foundations for science and revelation, he asserted that the evidence for the flood stood independently of revelation. Yet, he had then written, we nevertheless know that all truth is ultimately in accordance and it would be 'unreasonable to deny' a 'general coincidence' of science and revelation on the issue.

In the same section, however, we saw how in his later recantation in 1831, Sedgwick tried to pretend that the 'mistake' had arisen from a failure to apply the Baconian separation of science and revelation. On the specific flood issue, however, his 1831 comments on his new views are of sufficient interest in themselves for us to quote here the passages verbatim:

'Are then the facts of our science opposed to the sacred records? and do we deny the reality of a historic deluge? I utterly reject such an inference. Moral and physical truth may partake of a common essence, but as far as we are concerned, their foundations are independent, and have not one common element. And in the narrations of a great fatal catastrophe handed down to us, not in our sacred books only, but in the traditions of all nations, there is not a word to justify us in looking to any mere physical monuments as the intelligible records of that event: such monuments, at least, have not yet been found, and it is not perhaps intended that they

ever should be found ...<sup>38</sup>

Here, then, is his first suggestion. It is, ironically, not dissimilar to the view John Fleming had been espousing in the Edinburgh Journal since about 1824:

'... the simple narrative of Moses permits me to believe, that the waters rose upon the earth by degrees ... that the flood exhibited no violent impetuosity, displacing neither the soil nor the vegetable tribes which it supported ... With this conviction in my mind I am not prepared to witness in nature any remaining marks of the catastrophe, and I find my respect for the authority of revelation heightened, when I see, on the present surface, no memorials of the event.'<sup>39</sup>

The irony is that in his 1825 articles Sedgwick had defended Buckland against John Fleming's criticisms - though thinking Buckland 'far too secure in his position' to need such defence.

Sedgwick does not, however, rest content with this. His 1831 Address continues:

'But the facts recorded in history may sometimes, without confounding the nature of moral and physical truth, be brought into a general accordence with the known phenomena of nature: and such general accordence I affirm there is between our historical traditions and the phenomena of geology. Both tell us in a language easily understood, though written in far different characters, that man is a recent sojourner on the surface of the earth. Again, though we have not yet found the certain traces of any great diluvian catastrophe which we can affirm to be within the human period; we have, at least, shown that paroxsms of internal energy waves desolating whole

regions of the earth, were just a part of the mechanism of nature. And what has happened, again and again, from the most ancient, up to the most modern periods in the natural history of the earth, may have happened once during the few thousand years that man has been living on its surface.

We have therefore, taken away all anterior incredibility from the fact of a recent deluge ...'

Sedgwick, as may be seen, continued to be a catastrophist, though his ideas were now altered more in line with the theories of Elie de Beaumont on mountain chain elevation (as his Address makes clear). Lyell, writing in 1837 to Whewell, doubts that Sedgwick by then believes more than a small part of de Beaumont's system;<sup>4D</sup> though Sedgwick himself seems to make no direct reference to a change of view. He continues, however, to refer to Catastrophes (e.g. in the 1844 letter quoted above on Genesis). In the 1858 letter to Mrs. Cropper he writes:

'Land and sea have changed place. The tops of our highest hill have been under water. Therefore the fact of an historic deluge is not impossible or improbable ...'

This leaves a lot of unanswered questions. Did Sedgwick have in mind a limited flood? The idea was not new (Lyell traced it to Quirini in 1676) but he gives no indication of this. If it were still a universal flood, where did the water come from to cover the mountains? If it were a universal flood why had he not found evidence in the fossil record? To remove the anterior incredibility of a flood on the grounds that there was evidence for previous floods, only increased the posterior incredibility if there were no evidence for the one supposedly the most recent. The whole

argument seems to be poorly thought out. But it is all we have in his published works to indicate his views.

In theory, then, Sedgwick followed the Baconian viewpoint that the interpretation of revelation, and the inductive methods of science, rested on different bases, and should be attempted independently. There is no evidence that any kind of Biblical 'literalism' ever influenced his geological practice. The stories of creation and the flood did not make him try to fit geology into a particular mould, nor did he ever (in spite of the impression given by some to the contrary) make a basis of such literalism to object to evolution. On the other hand, however, he could not really insist that (on the issue of geology and the creation and flood) one did not impinge on the other. In this he always, throughout his long life, believed that they would ultimately harmonise. When it came to details, however, apart from a brief early period when he did link the Noachic flood with geology (and afterwards recanted), he was very wary of committing himself to any detail. He simply asserted that it was yet too early to tell. There may, however, be some evidence that (in spite of his unshaken confidence in ultimate harmony) his changing views about the distinctness of the epochs and the antiquity of man may have led him to perceive increasing difficulties in the gap-theory tentatively adopted. What his final views were, in default of more detailed material, we cannot tell.

## 7.6 Notes

1. The word 'moral' here has the special sense explained by Sedgwick in the passage cited below on p. 520: 'demoralised' does not mean either debauched or disheartened.
2. See p. 244 above (5th Edition Discourse, xix).
3. Letter to Darwin - see Chapter 7.5, note 89.
4. This section is only in the revised version (April 7th 1860) in The Spectator.
5. 1st Edition Discourse, p. 12.
6. e.g. 6th Edition, p. 246, etc.
7. See ref. 3.
8. See e.g. John Green, The Death of Adam, p. 10-13, 282-84 and 'Reflections on the Progress of Darwin Studies', Journal of History of Biology, 1975, 8, p. 246; Maurice Mendelbaum, 'Darwin's Religious Views', Journal of the History of Ideas, 1958, 19, 363-378, and History, Man and Reason, p. 85-87; Howard Gruber and Paul Barrett, Darwin on Man, p. 208-213; Edward Manier, The Young Darwin and His Cultural Circle, p. 68; Silvan Schweber, 'The Origin of the Origin Revisited', Journal of the History of Biology, 1977, 10, p. 233-34, 297 and 304-8; J.R. Moore, The Post Darwinian Controversies, p. 307-326; Dov Ospovat, 'God and Natural Selection', Journal of the History of Biology, 1980, 13, 169-194; C. Gillespie, Charles Darwin and the Problem of Creation. Another theme of some of these (e.g. in Mendelbaum) is over Darwin's attitude to materialism.
9. See 5th Edition Discourse, cxlviii.
10. Controversy surrounds the ideas e.g. of J. Monod, Chance And Necessity, etc.



11. See the quotation on following page.
12. This has been emphasized earlier in this present chapter and in chapter 5.6.
13. See chapter 4.2 and also later in the present chapter.
14. See above e.g. pp. 157-161.
15. See below in present chapter.
16. See below in present chapter.
17. 5th Edition Discourse, xiv; Sedgwick also says that Bacon would perhaps have qualified his censures if he had been aware of modern physiological discoveries. The quotation (from Bacon's Advancement of Learning, Bk. ii) applies the censures to Plato, Aristotle and Galen.
18. See above pp. 334-337.
19. The **third** and subsequent editions have 'fulness of its glorious light' instead of 'day spring from on high', but are otherwise unaltered.
20. 1st Edition Discourse, p. 104, 3rd Edition, p. 146, 5th Edn. p. 108.
21. 5th Edition Discourse, cclvii.
22. 'On Diluvial Formations', Annals of Philosophy, 1825, 10, p. 34.
23. 5th Edition Discourse, p. 142-43.
24. From the 1830 Presidential Address, p. 207.
25. See above part 6.1.2 etc.
26. 1st Edition Discourse, p. 105.
27. Sedgwick's reference is to the Scriptural Geologists.

28. 1st Edition Discourse, p. 107-8.
29. The letters to Sedgwick are in the Cambridge University Library, those to Whewell in the Trinity College; Gwatkin also corresponded with Henslow.
30. Clark & Hughes, 2, p. 79.
31. See 5th Edition Discourse, p. 115.
32. Gwatkin, writing to Sedgwick on 30th December 1854, takes it amiss that on p. 115 the new edition of the Discourse prints the passage about 'no good geologist' attempting an age-day theory, without any note or comment. Gwatkin may feel grieved for his own efforts, but he does mention Miller's lectures.
33. 5th Edition Discourse, cccv-cccv.
34. This is the manuscript letter - whether or not this was the version sent, it may at least indicate Sedgwick's views.
35. Clark & Hughes, 2, p. 344-45.
36. Ibid., p. 440.
37. See above chapter 7.3.
38. 1831 Address, p. 314.
39. Edinburgh Journal (April 1826).
40. Life, Letters and Journals of Sir Charles Lyell Bart., (Ed. K.M. Lyell) 2, p. 4.

## (8) A BRIEF EPILOGUE

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Adam Sedgwick was an important man in his own generation. As a geologist, he was one of the leaders in his field for some twenty years between about 1830 and the mid 1850's, and did a great deal to stimulate the stratigraphical discoveries then being made. In science in general, he was a founder of the Cambridge Philosophical Society (in 1819), and an early leader in the BAAS in the 1830's. In science education, his Discourse arguing for the value of what we would call science as a part of a liberal education helped pave the way for the introduction of the Natural Science Tripos by his friend Whewell in the 1850's.

His interest to us is in the fact that he is representative of a particular kind of scientist of the nineteenth century. His interests reflect the interests of many in his age, and he presents a particular view of science and scientific discovery which is within a total vision of a unified knowledge.

Yet, within the last twenty or so years, various wild statements seem to have been made about him, and many which are misleading. He has been called a 'liberal Anglican'. He has had the two extreme terms 'Broad Churchman' and 'Fundamentalist' both applied to him (and both anachronistically since neither had been invented in the period concerned). He has been called a semi-deist, and yet on the other hand accused of objecting to the Origin of Species on a basis of its 'open conflict with Scripture'. With one writer he narrowly escaped being classed in with the Scriptural Geologists. He has been pictured as a religious fanatic given to pious pronouncements, from whom Darwin must have been only too glad to escape during a

geological tour. He has been seen as a fabricator of elaborate technical myths. He has been accused of all kinds of very basic inconsistencies - in his scientific methodology, in his supposed religious bigotry, and in his mixture of the two.

Certainly Sedgwick was not without his faults, and to write a hagiography of the man would be as misleading as to load him with all the weight of the above statements. Nevertheless, the present thesis has shown that all the above are arguable, most are misleading, and many are more or less demonstrably incorrect. Sedgwick, viewed within the framework of his own times and his own beliefs, had a basic consistency running through his ideas and pronouncements. If at times he was liable to exaggerate during heated controversy, this does not negate that basic fact. I will conclude with some words about him which are perhaps not too far, at least, from the truth. These are from his epitaph:

ADAM SEDGWICK

A MASTER AMONG PHILOSOPHERS

THE FRIEND OF PRINCES, THE DELIGHT OF LITTLE ONES

AS ONE WHO

EXTENDED THE FRONTIERS OF SCIENCE

AND WAS FIRED WITH A RIGHT ROYAL LOVE OF TRUTH

WHOSE CHARACTER WAS A GRAND SIMPLICITY

AND WHOSE ROCK WAS THE FAITH OF CHRIST

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(9) MANUSCRIPT COLLECTIONS USED

A number of libraries and museums hold one or two letters of Sedgwick's, and where appropriate these have been referred to in the text. Seven main manuscript collections, however, have been used for this thesis. Brief descriptions of these are as follows:

(1) The British Library, London

A number of different collections in the British Library contain letters to and from Sedgwick; in particular, the papers of Gladstone, Sherbourne, Butter, and Macvey Napier contain many such letters.

(2) Cambridge University Library

This holds a large collection of letters, mainly to rather than from Sedgwick, with some copies of his letters to others.

(3) Cumbria County Archives, Kendal

This holds the parish records for Dent, and also some letters from Sedgwick to those of his home region.

(4) The Geological Society of London

The library of the Society holds a collection of Murchison notebooks and journals, letters from Sedgwick to Murchison, and copies of letters from Murchison to Sedgwick.

(5) The Sedgwick Museum, Cambridge

This houses the specimens collected by Sedgwick, his notebooks and journals, and some annotated books from his personal library.

(6) Trinity College Library, Cambridge

A collection known as the 'Whewell Manuscripts', containing

some 10,000 items, is held here, including many letters from Sedgwick to Whewell and to Whewell's second wife Lady Affleck.

(7) University Museum, Oxford

This holds a collection of letters to John Phillips, including many from Sedgwick.

At the beginning of this thesis my thanks were expressed to the archivists, curators, and librarians of the above institutions, for granting access to their collections and providing help in effective use of them. Thanks are also due for permission to cite from their sources as credited in the relevant notes, and in this respect ~~it may be added that~~ items from (1) are quoted by permission of the British Library, items from (2) by permission of the Syndics of Cambridge University Library, and items from (6) by permission of the Master and Fellows of Trinity College, Cambridge.

## (10) BIBLIOGRAPHY OF PUBLISHED SOURCES

### 10.1 Introduction

This bibliography contains the full details of all the published primary and secondary books and articles referred to in the thesis. Places of publication will be stated only if the work was not published in London.

A list of Sedgwick's works is given in a separate chapter 10.3. In addition, the following are standard works to which reference may have been made without necessarily citing editors or authors of individual articles:

The Dictionary of National Biography, Eds. Stephen, L. and

Lee, S., Oxford, 1885 (Compact Edition 1975 used).

The Encyclopaedia Britannica, 11th Ed., N. York, 1910-1911,

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The Dictionary of Scientific Biography, Ed. Gillespie, C.C.,

N. York, 1970-1980.

It may also be noted that in the thesis The Life and Letters of the Reverend Adam Sedgwick..., by Clark, J. and Hughes, T., is referred to as 'Clark & Hughes'. Sedgwick's own works are also referred to in shortened form, e.g. Discourse, Syllabus, Memorial.

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Valerius Terminus



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### 10.3 List of Sedgwick's Publications

Most of the published materials of Sedgwick taken in this thesis from secondary sources comes from The Life and Letters of the Reverend Adam Sedgwick, by J.W. Clark and T.M. Hughes (2 vols, Cambridge, 1890). Where I have been able to check their transcription against original letters, they appear to be accurate and reliable.

Clark & Hughes also give a fairly full list of Sedgwick's publications, which is reproduced below:

#### LIST OF SEDGWICK'S WORKS.

- 1820 ON the Physical Structure of those formations which are immediately associated with the Primitive Ridge of Devonshire and Cornwall. *Read 20 March, 1820.* *Trans. Phil. Soc. Camb., i. pp. 89—146.*
- 1821 ON the Physical Structure of the Lizard District in the County of Cornwall. *Read 2 April and 7 May, 1821.* *Trans. Phil. Soc. Camb., i. pp. 291—330.*
- A Syllabus of a course of lectures on Geology. 8vo. Camb., 1821. \*
- 1822 ON the Geology of the Isle of Wight. A Letter to the Editor of *The Annals of Philosophy*, dated Trinity College, Cambridge, 17 March, 1822. *Annals of Philosophy, N. S., iii. pp. 329—355.*
- ON the Phenomena connected with some Trap Dykes in Yorkshire and Durham. *Read 20 May, 1822.* *Trans. Phil. Soc. Camb., ii. pp. 21—44.*
- 1823 Letter to the Members of the Senate, dated Trin. Coll., 14 May, 1823. *The Cambridge Chronicle, 23 May, 1823.*
- A reply to an Address to the Senate, published by the Master of Jesus College. 8vo. Cambridge, 1823, pp. 86.
- 1823-24 ON the Association of Trap Rocks with the Mountain Limestone Formation in High Teesdale, &c. *Read 12 May, 1823; 1 March and 15 March, 1824.* *Trans. Phil. Soc. Camb., ii. pp. 139—195.*
- 1824 Remarks on the observations of Dr French; with an argument on the law of elections to offices created by the Senate. Dated Trin. Coll., Feb. 25, 1824. 8vo. Cambridge, pp. 48.
- 1825 ON the Origin of Alluvial and Diluvial Formations. A Letter to the Editor of *The Annals of Philosophy*, dated Trinity College, Cambridge, 11 March, 1825. \*
- Annals of Philosophy, N. S., ix. 1825, pp. 241—257.*

On Diluvial Formations. A letter dated May, 1825.

*Annals of Philosophy*, x. 1825, pp. 18—37. \*

The Minutes of the Phil. Soc. Camb. shew that a paper "On the essential distinction between Alluvial and Diluvial Deposits," had been read to the Society, 21 February and 7 March.

"A portion of a paper was read by Professor Sedgwick on the Geology of the Yorkshire Coast, a section of which was exhibited to the Society."

*Minutes, Phil. Soc. Camb.*, 2 May, 1825.

The paper was completed 16 May (*Ibid*).

- 1826 On the Classification of the Strata which appear on the Yorkshire Coast. A Letter to the Editors of the *Annals of Philosophy*, dated 20 Feb., 1826, followed by a Memoir. Issued with separate title-page, as above, and "London: printed by C. Baldwin, New Bridge-Street, 1826." *Read to Phil. Soc. Camb.*, 2 May, 16 May, 1825.

*Annals of Philosophy*, N. S., 1826, xi. pp. 339—362.

"A paper was read by Prof. Sedgwick 'on the Geology of the Isle of Wight'."

*Minutes, Phil. Soc. Camb.*, 8 May, 1826.

"Rev. Prof. Sedgwick exhibited to the Society a pair of large horns of some species of the genus *Bos* found near Walton in Essex."

*Minutes, Phil. Soc. Camb.*, 13 November, 1826.

- 1826—28 On the Geological Relations and Internal Structure of the Magnesian Limestone, and the lower Portions of the New Red Sandstone Series in their Range through Nottinghamshire, Derbyshire, Yorkshire, and Durham, to the Southern extremity of Northumberland. *Read 17 Nov.*, 1826; *30 April*, 18 May, 1827; *7 March*, 1828.

*Trans. Geol. Soc. Lond.*, Ser. 2, iii. pp. 37—124.

- 1827 "After the meeting Professor Sedgwick gave an account of the peculiarities of the Coal Strata in the neighbourhood of Whitehaven."

*Minutes, Phil. Soc. Camb.*, 14 May, 1827.

- 1828 On the Geological Relations of the Secondary Strata in the Isle of Arran. By Rev. A. Sedgwick [etc.], and R. I. Murchison. *Read 18 January and 1 February*, 1828.

*Trans. Geol. Soc. Lond.*, Ser. 2, iii. pp. 21—36.

"After the meeting Prof. Sedgwick gave an account of the Geological Structure of Scotland as collected from the observations made by himself and Mr Murchison during the preceding summer."

*Minutes, Phil. Soc. Camb.*, 3 March, 1828.

On the Structure and Relations of the Deposits contained between the Primary Rocks and the Oolitic Series in the North of Scotland. By the Rev. A. Sedgwick [etc.], and R. I. Murchison [etc.]. *Read 16 May and 6 June*, 1828.

*Trans. Geol. Soc. Lond.*, Ser. 2, iii. pp. 125—160.

- 1829 Letter to Right Hon. H. Goulburn. 3 June. Signed: *A Resident Member of the Senate*.

"After the meeting Professor Sedgwick gave an account of the Geological Structure of the Alps, illustrated by a section passing from the plains of Bavaria to those of Trieste."

*Minutes, Phil. Soc. Camb.*, 30 Nov., 1829.

- 1829-30 A Sketch of the Structure of the Eastern Alps; with sections through the Newer Formations on the Northern Flanks of the Chain, and through the Tertiary Deposits of Styria, etc., etc. By Rev. A. Sedgwick [etc.] and R. I. Murchison [etc.]. Read 6 Nov., 20 Nov., 4 Dec., 1829; 5 March, 1830.

With Supplementary Observations, Sections, and a Map. By R. I. Murchison, [etc.]. Read 19 Jan. and 2 Feb., 1831.

*Trans. Geol. Soc. Lond., Ser. 2, iii. pp. 301-420.*

- 1830 Address delivered [as President] at the Anniversary Meeting of the Geological Society of London, 19 February, 1830. 8vo. London, pp. 28. \*

*Proc. Geol. Soc. Lond., i. pp. 187-212.*

- A Sketch of the Structure of the Austrian Alps. By Rev. A. Sedgwick and R. I. Murchison. Read 21 May, 1830.

*Proc. Geol. Soc. Lond., i. pp. 227-231.*

- 1831 Introduction to the General Structure of the Cumbrian Mountains; with a Description of the great Dislocations by which they have been separated from the neighbouring Carboniferous Chains. Read 5 January, 1831. With an Appendix. Read 6 November, 1833.

*Trans. Geol. Soc. Lond., Ser. 2, iv. pp. 47-68\*.*

- Address on announcing the first award of the Wollaston Prize. Delivered 18 February, 1831. \*

*Proc. Geol. Soc. Lond., 1831, i. pp. 270-279.*

- Address to the Geological Society, delivered on the evening of the 18th of February, 1831. on retiring from the President's Chair. \*

*Proc. Geol. Soc. Lond., 1831, i. pp. 281-316.*

- Description of a Series of Longitudinal and Transverse Sections through a Portion of the Carboniferous Chain between Penigent and Kirkby Stephen. Read 2 March and 16 March, 1831.

*Trans. Geol. Soc. Lond., Ser. 2, Vol. iv. pp. 69-102.*

- Address to Senate in opposition to the claims of Mr Goulburn and Mr W. Y. Peel (2 May). Signed: *A Resident Member of the Senate.*

- "After the meeting Professor Sedgwick gave an account illustrated by sections, of the geological structure of Caernarvonshire."

*Minutes, Phil. Soc. Camb., 14 Nov., 1831. Phil. Mag. 1832, 69.*

- 1832 On the New Red Sandstone Series in the Basin of the Eden, and North-western Coasts of Cumberland and Lancashire. Read 1 February, 1832.

*Trans. Geol. Soc. Lond., Ser. 2, iv. pp. 383-408. Proc. Geol. Soc. Lond. i. pp. 343-345, entitled: On the deposits overlying the carboniferous series in the valley of the Eden, and on the north-western coasts of Cumberland and Lancashire. Phil. Mag., xi. pp. 283-285.*

- "After the meeting Professor Sedgwick gave an account, illustrated by maps and sections, of the Physical Geography and History of the Fens of Cambridgeshire."

*Minutes, Phil. Soc. Camb., 7 May, 1832.*

- On the Geological Relations of the Stratified and Unstratified Groups of Rocks composing the Cumbrian Mountains. Read 16 May, 1832.

*Proc. Geol. Soc. Lond., i. 399-401.*

- Verbal account of the Geology of Caernarvonshire.

*Report, Brit. Ass. (Oxford), June 1832, p. 583.*

- Remarks on Mineral Veins.

*Ibid. p. 579.*

Note on certain fossil shells overlying the London clay in the Isle of Sheppey. *Read 21 November, 1832.*

*Proc. Geol. Soc. Lond.*, i. 409.

A Syllabus of a course of lectures on Geology. 2nd Edition. 8vo. Camb., 1832. \*

1833 "After the meeting Professor Sedgwick gave an account, illustrated by representations of sections, of the Geology of North Wales."

*Minutes, Phil. Soc. Camb.*, 11 March, 1833. *Phil. Mag.*, 1833, ii. 381.

"Professor Sedgwick described the leading features in the Geology of North Wales, the lines of elevation, the relation of the trap rocks to the slate-system, the cleavage of the slate; pointed out the relations of this tract to that examined by Mr Murchison: and drew a general parallel between the slate formations of Wales and Cumberland."

*Report Brit. Ass. (Cambridge)*, 1833, p. xxxiii.

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*Ibid.*

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*Proc. Geol. Soc. Lond.*, ii. 1.

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Letter addressed "To the Resident Members of the Senate." Dated Trin. Coll. 16 April, in defence of Professor Hewett.

Printed in *The Cambridge Chronicle*, 18 April.

Seventeen Reasons for adopting the Prayer of the Petition signed by sixty-two resident Members of the Senate. \*

*The Times*, probably about 18 April.

Letter to Editor of *Cambridge Independent Press*, dated Trin. Coll. 18 April, in reply to a letter signed *A Member of the Senate* [Dr French], printed in *The Cambridge Chronicle*, 11 April.

Remarks on Mr Beverley's Letter, etc. Third Letter to the Editors of the *Leeds Mercury*. Dated Trin. Coll. 15 May.

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Letter "To the Members of the Senate," in reply to Mr Selwyn. Dated Trin. Coll. 9 June.

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*Minutes, Phil. Soc. Camb.*, 24 Nov. 1834.
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 8vo. Camb., 1834.
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- 1835 Remarks on the Structure of large Mineral Masses, and especially  
 on the Chemical Changes produced in the Aggregation of  
 Stratified Rocks during different Periods after their Deposition.  
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*Trans. Geol. Soc. Lond.*, iii. pp. 461—486. *Karsten, Archiv*, x. pp. 581—626.
- Account of a field-lecture.  
*The Cambridge Chronicle*, 10 April, 1835.
- On the range of the Carboniferous Limestone flanking the primary  
 Cumbrian Mountains; and on the Coal-fields of the N.W. coast  
 of Cumberland, etc. By A. Sedgwick and Williamson Peile,  
 Esq. of Whitehaven. *Read 10 June, 1835.*  
*Proc. Geol. Soc. Lond.*, ii. pp. 198—200.
- On the Silurian and Cambrian Systems, exhibiting the order in  
 which the older Sedimentary Strata succeed each other in  
 England and Wales. By Professor Sedgwick and R. I.  
 Murchison.  
*Report, Brit. Ass. (Dublin)*, Part ii. pp. 59—61.
- Extrait d'une lettre à M. Élie de Beaumont sur le Développement  
 des Roches stratifiées anciennes dans le Cumberland et le Pays  
 de Galles.  
*Bull. Soc. Géol. de France*, vii. pp. 152—155.
- A Discourse on the Studies of the University. Fourth Edition.  
 8vo. Camb., 1835.
- 1836 "Professor Sedgwick gave an account of the system of formations  
 inferior to the Carboniferous Series, as illustrated by his own  
 researches in Wales, and those of Mr Murchison in the same  
 country."  
*Minutes, Camb. Phil. Soc. Camb.*, 18 April, 1836.
- On the Coal-fields on the North-western coast of Cumberland. By  
 A. Sedgwick and Williamson Peile, Esq. of Whitehaven. *Read*  
*8 June, 1836.*  
*Proc. Geol. Soc. Lond.*, ii. pp. 419—422.
- A Classification of the old Slate Rocks of the North of Devonshire,  
 and on the true position of the Culm Deposits in the central  
 portion of that County. By Rev. A. Sedgwick and R. I.  
 Murchison.  
*Report, Brit. Ass. (Bristol)*, Part ii. pp. 95, 96.
- Description of a Raised Beach in Barnstaple or Bideford Bay, on  
 the North-West Coast of Devonshire. By A. Sedgwick and  
 R. I. Murchison. *Read 14 December, 1836.*  
*Trans. Geol. Soc. Lond.*, v. pp. 279—286.
- Four Letters to the Editors of the *Leeds Mercury* in reply to  
 R. M. Beverley, Esq. [Not published.]  
 8vo. Cambridge, 1836. pp. 64.
- 1837 On the Physical Structure of Devonshire, and on the Subdivisions  
 and Geological Relations of its older Stratified Deposits, etc.  
 Part I. By A. Sedgwick [etc.], and R. I. Murchison [etc.].  
*Read 14 June, 1837.*  
*Trans. Geol. Soc. Lond.*, v. pp. 633—687.
- Notice of an Incursion of the Sea into the Collieries at Workington.  
*Report, Brit. Ass. (Liverpool)*, Part ii. pp. 75, 76.

"Professor Sedgwick gave an account of the Geology of Charnwood Forest, and the neighbouring coal-fields."

*Minutes, Phil. Soc. Camb.*, 13 Nov. 1837.

A Syllabus of a course of Lectures on Geology. 3rd Edition.

- 1838 Petition to the House of Commons against a Bill to carry into effect the Fourth Report of the Commissioners of Ecclesiastical Duties and Revenues.

[Undated: but sealed at a meeting of the Dean and Chapter of Norwich held 14 May, 1838. It was then printed and circulated among members of the House of Commons.]

A Synopsis of the English Series of Stratified Rocks inferior to the Old Red Sandstone—with an attempt to determine the successive natural groups and formations. *Read 21 March and 23 May, 1838.*

*Proc. Geol. Soc. Lond.*, ii. pp. 675—685.

- 1839 Classification of the Older Stratified Rocks of Devonshire and Cornwall. By the Rev. Professor Sedgwick [etc.] and R. I. Murchison [etc.]. Dated 25 March, 1839.

*Phil. Mag. Ser. 3, Vol. 14, pp. 241—260.*

Postscript to the communication of Prof. Sedgwick and Mr Murchison in the present number at p. 241. [n. d.]

*Ibid. p. 317.*

Supplementary Remarks on the "Devonian" System of Rocks. By the Rev. Professor Sedgwick [etc.] and R. I. Murchison [etc.]. April 19, 1839.

*Phil. Mag. Ser. iii. Vol. 14, pp. 354—358.*

"Professor Sedgwick on Geology of Cornwall and Devon."

*Minutes, Phil. Soc. Camb.* 22 April, 1839.

"A Communication respecting the Geology of Northern Germany East and West of the Rhine."

*Minutes, Phil. Soc. Camb.*, 25 Nov. 1839

On the Physical Structure of Devonshire etc. Part II. By A. Sedgwick [etc.] and R. I. Murchison [etc.]. *Read 24 April, 1839.*

*Proc. Geol. Soc. Lond.*, iii. pp. 121—123. *Trans. Geol. Soc. Lond.*, v. pp. 688—703\*.

- 1840 On the Distribution and Classification of the older or Palæozoic Deposits of the North of Germany and Belgium, and their comparison with Formations of the same age in the British Isles. *Read 13 May and 27 May, 1840.* \*

By A. Sedgwick [etc.] and R. I. Murchison [etc.]. Followed by: Description of the Fossil Mollusca, by Viscount D'Archiac and M. E. de Verneuil, Members of the Geological Society of France, etc. etc.

*Trans. Geol. Soc. Lond.*, vi. pp. 221—410. Issued with separate Title-page as above, and "London, printed by Richard and John E. Taylor, Roper's Court, Fleet Street, 1842."

- 1841 Two letters to the Editor of *The Norfolk Chronicle* in answer to correspondent signing himself Miles. 27 April and 10 May 1841.

Supplement to a "Synopsis of the English Series of Stratified Rock inferior to the Old Red Sandstone," with additional remarks on the Relations of the Carboniferous Series and Old Red Sandstone of the British Isles. *Read 3 November and 17 November 1841.*

*Proc. Geol. Soc. Lond.*, iii. Pt. ii. pp. 545—55.

"Professor Sedgwick gave an account of the comparative classification of the older strata of the British Isles."

*Minutes, Phil. Soc. Camb.*, 15 Nov., 1841

Circular to Members of the Senate soliciting subscriptions to buy the Whitby *Plesiosaurus*. 19 November, 1841.

- 1842 "Professor Sedgwick gave an account of Professor Owen's memo on the skeleton of the *Mylodon*, and on the structures and habits of certain extinct genera of Fossil Sloths."

*Minutes, Phil. Soc. Camb.*, 20 March, 184

Three letters on the Geology of the Lake District addressed to W. Wordsworth, Esq. (23 May, 24 May, 30 May). Printed in *A Complete Guide to the Lakes*, by John Hudson, Kendal.

- 1843 Outline of Geological Structure of North Wales. Read 21 June, 184.  
*Proc. Geol. Soc. Lond.*, iv. Pt. i. pp. 212—22.

"Professor Sedgwick gave an account of the structure and relations of the slate rocks of North Wales."

*Minutes, Phil. Soc. Camb.*, 13 Nov., 1843.

On the Older Palæozoic (*Protozoic*) Rocks of North Wales. Read 15 November, and 29 November, 1843.

[A continuation of the paper on Geological Structure of N. Wales, read 21 June, 1843.]

*Quart. Journ. Geol. Soc. Lond.*, i. pp. 5—22.

- 1844 Notes appended to *Notice on the Occurrence of Land and Freshwater Shells with Bones of some extinct Animals in the Gravel near Cambridge*. By P. B. Brodie, F.G.S., Emmanuel College. The notes are dated 8 March, 1844.

*Trans. Phil. Soc. Camb.*, viii. 139.

"Professor Sedgwick gave a sketch of the progress of discovery among the older stratified geological deposits of Britain."

*Minutes, Phil. Soc. Camb.*, 11 Nov., 1844.

A communication on the geology of North Wales in continuation of the Memoir read 15 November and 29 November, 1843. (29 May). No abstract of this paper is given.

*Quart. Journ. Geol. Soc. Lond.*, i. 214.

- 1845 On the comparative Classification of the Fossiliferous Strata of North Wales, with the corresponding deposits of Cumberland, Westmoreland, and Lancashire. Read 12 March, 1845.

*Quart. Journ. Geol. Soc. Lond.*, i. pp. 442—450.

On the Geology of the Neighbourhood of Cambridge, including the Formations between the Chalk Escarpment and the Great Bedford Level. Read at Cambridge Meeting of Brit. Ass. June, 1845.

*Brit. Ass. Rep. (for 1845) Pt. 2, pp. 40—47.*

"Professor Sedgwick gave an account of the fossiliferous slate rocks of the Lake Mountains."

*Minutes, Phil. Soc. Camb.*, 24 Nov., 1845.

- 1846 On the Classification of the Fossiliferous Slates of Cumberland, Westmoreland, and Lancashire (being a supplement to a paper read to the Society, March 12, 1845). Read 7 January and 21 January, 1846.

*Quart. Journ. Geol. Soc. Lond.*, ii. pp. 106—131.

Supplemental letter to Wordsworth on Geology of Lake District. Cambridge, 30 May [Letter iv]. Published in Hudson's *Guide*. \*

On the Classification of the Fossiliferous Slates of North Wales, Cumberland, Westmoreland, and Lancashire (being a supplement to a paper read to the Society, March 12, 1845). Read 16 December, 1846.

*Quart. Journ. Geol. Soc. Lond.*, iii. 133—164.

Geology of the Lake District, in four letters addressed to W. Wordsworth, Esq. \*

Printed in '*A complete Guide to the Lakes*,' edited by the publisher.

- 1847 "Professor Sedgwick made a communication on the geology of North and South Wales."

*Minutes, Phil. Soc. Camb.*, 15 March, 1847.



- 1848 On the Organic Remains found in the Skiddaw Slate, with some Remarks on the Classification of the Older Rocks of Cumberland and Westmoreland, etc. *Read 2 February, 1848.*

*Quart. Journ. Geol. Soc. Lond., iv. pp. 216—225.*

"Professor Sedgwick gave a lecture on the structure of the southern mountain-chain of Scotland, compared with that of the neighbouring parts of Cumberland."

*Minutes, Phil. Soc. Camb., 12 Feb., 1849.*

- 1850 A Discourse on the Studies of the University of Cambridge. The fifth edition, with additions, and a preliminary dissertation. 8vo. Camb. 1850. The preface is dated 30 November, 1849. \*

8vo. Camb., 1850.

On the Geological Structure and Relations of the Frontier Chain of Scotland.

*Report, Brit. Ass. (Edinburgh), Part ii. pp. 103—107.*

"Professor Sedgwick gave a lecture on the Gulf Stream, and its effects on the climate of the British Isles."

*Minutes, Phil. Soc. Camb., 25 Nov., 1850.*

- 1851 Reply to Sir Charles Lyell (President of the Geological Society) on receiving the Wollaston Medal, 21 February.

*Quart. Journ. Geol. Soc. Lond., vii. xx.*

A Synopsis of the Classification of the British Palæozoic Rocks. First *Fasciculus*. Advertisement dated May, 1851.

Letter to the Editor of the *Morning Chronicle* on the case of the Rev. Philip Bland (17 May).

A second letter on the same subject (28 May).

On the Slate Rocks of Devon and Cornwall. *Read 5 November, 1851.*

*Quart. Journ. Geol. Soc. Lond., viii. pp. 1—19.*

"Professor Sedgwick gave an account of certain phenomena observed at the junction of the carboniferous chain of Yorkshire and Westmoreland with the Cambrian and Silurian rocks."

*Minutes, Phil. Soc. Camb., 24 Nov., 1851.*

On the Lower Palæozoic Rocks at the Base of the Carboniferous Chain between Ravenstonedale and Ribblesdale. *Read 3 December, 1851.* \*

*Quart. Journ. Geol. Soc. Lond., viii. pp. 35—54.*

- 1852 On the Classification and Nomenclature of the Lower Palæozoic Rocks of England and Wales. *Read 25 February, 1852.* \*

*Quart. Journ. Geol. Soc. Lond., viii. pp. 136—168.*

Reply to an article by Sir R. I. Murchison (*Lit. Gazette*, 20 March, p. 278). \*

*Literary Gazette*, 10 April, p. 338.

Second article on same subject (in reply to a second letter by Sir R. I. Murchison, *Lit. Gazette*, 24 April, p. 369), dated Norwich, May 8. \*

*Literary Gazette*, 15 May, p. 417.

A Synopsis of the Classification of the British Palæozoic Rocks. Second *Fasciculus*. Advertisement dated July 16, 1852. \*

On a proposed separation of the so-called Caradoc Sandstone into two distinct groups; viz. (1) May Hill Sandstone; (2) Caradoc Sandstone. *Read 3 November, 1852.* \*

*Quart. Journ. Geol. Soc. Lond., ix. pp. 215—230.*

Answers from the Rev. Adam Sedgwick, M.A. Woodwardian Professor of Geology.

*Report of Her Majesty's Commissioners appointed to inquire into the State, Discipline, Studies, and Revenues of the University and Colleges of Cambridge*, Fol. Lond. 1852. Evidence, pp. 115—121.

Recollections of Henry Bickersteth, afterwards Lord Langdale, while at Cambridge.

Printed in *Memoirs of the Right Honourable Henry Lord Langdale*. By Thomas Duffus Hardy. 2 vols. 8vo. Lond. 1852. i. 231—234.

- 1853 Supplemental letter on Geology of Lake district [Fifth of the series], addressed to Mr J. Hudson, Bookseller, Kendal. [Letter v.] Cambridge, 23 June, 1853. \*

On the Classification and Nomenclature of the older Palæozoic Rocks of Britain. \*

*Report, Brit. Ass. (Hull), Part ii. pp. 54—61.*

Geology of the Lake district of Cumberland, Westmoreland, and Lancashire, in [five] letters addressed to W. Wordsworth, Esq. 12mo. Kendal, John Hudson, 1853.

Printed in *A complete Guide to the Lakes*, edited by the publisher.

- 1854 A Reply to two Statements published by the Palæontographical Society, in their volume for 1853; one appearing to accuse the University of Cambridge of illiberality in the administration of its Museum; the other reflecting on the character of Professor McCoy. [A letter dated 4 March, 1854]

*Ann. and Mag. Nat. Hist., Ser. 2, Vol. xiii. pp. 280—292.*

On the May Hill Sandstone, and the Palæozoic System of England. [Letter dated 20 August, 1854, followed by a memoir.] \*

The paper read to the Geol. Soc. Lond. 3 May, 1854, but not published in their *Journal*.

*Phil. Mag., Ser. 4, viii. pp. 301—317; 359—370.*

Rejoinder to Professor Milne-Edwards and Mr Bowerbank. [A letter dated 21 August, 1854.]

*Ann. and Mag. Nat. Hist., Ser. 2, Vol. xiv. pp. 195—197.*

On the May Hill Sandstone and Palæozoic Systems of England. [Letter dated 31 October, 1854.]

This is the paper read to the British Association at Liverpool, September, 1854. *Brit. Ass. Rep. Sections, p. 95.*

*Phil. Mag., Ser. 4, viii. pp. 472—506.*

- 1855 "On the classification and nomenclature of the Palæozoic rocks."

*Minutes, Phil. Soc. Camb., 12 Nov. 1855.*

A Lecture, delivered at The Athenæum, Bury St Edmund's, December 19th, 1854, on the natural affinities and habits of the extinct Gigantic Sloths, by Professor Sedgwick.

Bury St Edmund's: Printed for the Bazaar, by W. P. Jackson, Angel Hill. 8vo. Lond. pp. 38.

A Synopsis of the Classification of the British Palæozoic Rocks, by Rev. A. Sedgwick: with a systematic description of the British Palæozoic Fossils in the Geological Museum of the University of Cambridge, by Frederick McCoy, F.G.S. etc. 4to. London and Cambridge, 1855. \*

- 1856 Letter to the Members of the Senate soliciting subscriptions for the purchase of a collection of fossils formed by Rev. T. Image. (10 January.)

Letter to the Editor of *The Cambridge Chronicle* on the same subject (20 February).

- 1857 Remarks on a passage in the President's Address delivered at the Anniversary Meeting of the Geological Society of London, on the 15th of February, 1856. [Letter dated 9 January, 1857, followed by memoir.]

*Phil. Mag., Ser. 4, Vol. xiii. pp. 176—182.*

Description of a series of dislocations which have moved the Cambrian and Silurian Rocks between Leven Sands and Duddon Sands. *Read to Phil. Soc. Camb. 9 Nov. 1857.*

*Phil. Mag., Ser. 4, Vol. xvi. pp. 155—158. Proc. Phil. Soc. Camb., i. pp. 187—190.*

- 1858 Farewell Letter to Dr Livingstone (10 February).  
Printed in *The Personal Life of David Livingstone*. By W. G. Blaikie, D.D. 8vo. Lond. 1880, p. 238. \*
- On Faults in Cumberland and Lancashire.  
*Report, Brit. Ass. (Aberdeen) Part ii.*, p. 265. *Title only.*
- 1860 Article on *The origin of Species*, originally written in the form of a letter to the Archbishop of Dublin. Not signed. \*
- The Spectator*, 24 March, p. 285; revised, with some additions, 7 April, p. 344.
- On the Succession of Organic forms during long geological periods; and on certain Theories which profess to account for the origin of new species. *Read 7 May.*  
*Proc. Phil. Soc. Camb.* i. 223.
- A synopsis, supplied "by the courtesy of the Professor" is printed in *The Cambridge Chronicle*, 19 May.
- A Lecture on the Strata near Cambridge and the Fens of the Bedford Level, delivered to the members and friends of the Working Men's College, and of the Young Men's Christian Association, at the Town Hall, Cambridge, 1 June, 1860. *Not published.* With map and supplement.
- The preface is dated 1 December, 1861. The lecture was really delivered 25 May, 1860, not 1 June.
- On the Geology of the Neighbourhood of Cambridge and the Fossils of the Upper Greensand.  
*Report, Brit. Ass. (Oxford), Part ii.* p. 101. (*Title only.*)
- 1863 Letter to the Editor of *The Cambridge Chronicle* describing the career and death of Mr Lucas Barrett. Dated Norwich, 19 January, 1863.
- 1865 A Sketch of the Geology of the Valley of Dent, with some account of a destructive Avalanche which fell in the year 1752. *Read 13 November, 1865.*  
*Proc. Phil. Soc. Camb.*, i. pp. 236—238.
- 1867 Report on the Woodwardian Museum.  
*First Annual Report of Museums and Lecture Rooms Syndicate*, 1 April, 1867.
- Impressions of the character of Prince Albert: in a Letter to General Grey. See *Preface to The Early Years of His Royal Highness the Prince Consort*. 8vo. Lond., 1867.
- 1868 Report on the Woodwardian Museum.  
*Second Annual Report of Museums and Lecture Rooms Syndicate*, 24 February, 1868.
- A Memorial by the Trustees of Cowgill Chapel, with a preface and appendix, on the climate, history, and dialects of Dorset. 8vo. Cambridge, 1868. [*Printed for private circulation.*] \*
- Letter of condolence on the death of Rev. H. V. Elliott. Printed in *Life of Rev. H. V. Elliott*. By Jos. Bateman, M.A. 8vo. Lond., 1868, p. 374.
- 1869 Report on the Woodwardian Museum (9 March).  
*Third Annual Report of Museums and Lecture Rooms Syndicate*, dated 16 March.
- Prefatory Notice (dated 20 August), to *Index to the Fossil Remains of Aves, Ornithosauria, and Reptilia...in the Woodwardian Museum*. By Harry Govier Seeley. 8vo. Camb., 1870.
- 1870 Supplement to the Memorial of the Trustees of Cowgill Chapel, with an Appendix, etc., printed in 1868. Printed for private circulation only. 8vo. Cambridge, 1870. \*
- Report on the Woodwardian Museum (29 March).  
*Fourth Annual Report of the Museums and Lecture Rooms Syndicate*, dated 21 May.
- Report on the Woodwardian Museum (11 March).  
*Fifth Annual Report of the Museums and Lecture Rooms Syndicate*, dated 31 March.

- 1871 Printed circular soliciting subscriptions for the purchase of the Leckenby Collection (23 November).
- 1872 Report on the Woodwardian Museum.  
*Sixth Annual Report of the Museums and Lecture Rooms Syndicate, dated 21 March.*
- 1873 A Catalogue of the collection of Cambrian and Silurian Fossils contained in the Geological Museum of the University of Cambridge, by J. W. Salter, F.G.S. With a preface by the Rev. Adam Sedgwick; and a table of genera and index added by Professor Morris. Cambridge, 4to. 1873.  
Sedgwick's preface is dated 17 September, 1872.

The items marked with a star are those of especial interest for this thesis. Clark & Hughes only significant omission is the anonymous review of Vestiges of the Natural History of Creation, which appeared in the Edinburgh Review, July 1845, 165, pp. 1-85. As there is no doubt that Sedgwick was its author, no particular note has been taken in my thesis of its anonymous nature, and it is referred to simply as 'Review of the Vestiges'. The following other abbreviations have also been adopted:

Syllabus: A Syllabus of a course of lectures on Geology,  
(1821, 1832, 1837).

President's Address: Sedgwick gave two important addresses to the Geological Society, one in 1830 (Proc. Geol. Soc. Lond., 1, 187-212) and one in 1831 (Proc. Geol. Soc. Lond., 1, 281-316).

Discourse: A Discourse on the Studies of the University,  
(1833, 1834, 1834, 1835, 1850).

Memorial: A Memorial by the Trustees of Cowgill Chapel,  
(1868, Supplement 1870).